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and
Accurate
Mechanism

T. N.
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THE DESIGN AND USE OF INSTRUMENTS AND ACCURATE MECHANISM

UNDERLYING PRINCIPLES

BY

T. N. WHITEHEAD
Assistant Professor, Harvard School of Business Administration
Late Scientific Officer to the British Admiralty

NEW YORK
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1934

The Design and Use of
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by T. N. Whitehead

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PREFACE

The present book is written primarily for designers of instruments and for those who use them, but the subject matter really transcends these limits and some few of the principles involved are equally applicable to mechanism as a whole, whilst still more are germane to a large class of light machinery which has many of the essential characteristics of an instrument—thus, clock makers' precision lathes; carburetors and magnetos of internal combustion engines; many components of textile machinery; the design of governors; automatic and hand controls of all sorts; subsidiary parts of printing presses; to mention only a few examples.

The most casual examination of heavy machinery reveals the fact that it tends to fall naturally into two sections: a central unit which crudely delivers power in some predetermined manner, surrounded by a host of auxiliary mechanism which feed and control this central machine; these auxiliaries are the nerves and coördinating centres of the body, and without undue metaphor might be described as instruments ministering to machinery rather than man. Hence, although some parts of the book are applicable only to instruments in the strict sense, the examples chosen will in many cases show the similarity or identity of the more general principles of machine design.

The day has gone by when the maker of "philosophical" instruments, as they were called, was an artist mechanic who achieved, by incredible labor and skill, a unique instrument with its individual merits and defects; not only do we require an accuracy often far beyond his powers, but this must be achieved in the face of a drop in manual skill and at a smaller cost of human effort. Two factors coöperate to render this possible, the precision machine tool and scientific design; curiously enough the latter is almost more important than the former, for with an adequate design it is possible to make many measuring instruments with the help of a few hand tools better than an obsolete instrument with all the resources of a modern workshop. Some instruments, such as watches, are wanted in sufficient numbers to be mass produced in the strict sense of the word, but for the most part this does not quite apply to the better grade of instruments; for, although in the aggregate a surprising proportion of human energy is devoted to producing accurate instruments and light controlling mechanism of one sort or another, nevertheless, the diversity is such that no one item commands a very wide market; moreover, the increasing pace of invention, and particularly of improvement, renders the effective life of a given instrument correspondingly short.

The growing importance of these factors necessitates a revision in the outlook of the designer, and the increasing variety of problems suggests that he must rely less on a detailed knowledge of particular examples and special devices than on a body of underlying principles which will serve to guide him in novel situations. This is, of course, the basis of all organized knowledge, but as the problems become increasingly more diverse and less stable so must the principles be more fundamental and carefully based if they are to cover the wider field.

It must have occurred to every one conversant with the literature of engineering, and of instrument design in particular, that, in spite of tremendous expansion, the basic principles have been singularly stable for a very considerable time; this is a tribute to the ability of those engineers in the past who laid these foundations, but it is also a warning that the time has come to take stock of the position. The standard basis of engineering theory seems to require revision in two respects; first, by taking advantage of recent research in other branches of science which have reached the stage of practical application, and secondly, by subjecting the data from which the theory is deduced to a critical examination.

Mathematics is the natural means by which physical laws are deduced from the raw material of experience; but, although exceedingly powerful in some respects, mathematical procedure demands rather simple data of a peculiar type; thus, it is ill adapted to deal with discontinuities, and postulates such abstractions as oceans of infinite area, solids of perfect elasticity, ideal liquids, and so forth. Moreover, the mathematician can only cope with data capable of quantitative expression. Consequently, the mathematician lives in an ideal world, and the practical importance of his conclusions must depend on the essential similarity of his world to ours. Now as practical engineers it is our business to assess this similarity and never to mistake the mathematical abstraction for reality; but this is exactly what the engineer is apt to do; from his comparative lack of mathematical facility he too readily accepts the conclusions of a train of reasoning without sufficiently examining the relevance of its assumptions, a subject on which he and not the mathematician is the expert. This has resulted in the over-abstraction of many branches of engineering theory, such as the application of the theory of probability to errors of observations; these errors being assumed, in spite of the evidence, to be in line with those postulated by the mathematicians. The same tendency can be seen in typical discussions of "kinematic design". Thus, the present book represents a conscious revolt against mathematical abstraction when unaccompanied by an independent examination of the actual facts.

The general principles considered in this book are not intended to be exhaustive; to mention only one example;

all direct discussion of the applied theory of elasticity is omitted, for the university-trained engineer has received a thorough grounding in this application. The aim has been rather to concentrate on those parts of the subject which seem to call for revision and with which mechanical designers are likely to be less acquainted.

I have the pleasant duty of acknowledging my indebtedness to the following firms for their courtesy in supplying me with both illustrations and information relating to various instruments made by them: Messrs. Adam Hilger, Ltd.; the Cambridge Instrument Company, Ltd.; the Société Genevoise d'Instruments de Physique; Messrs. Alfred Herbert, Ltd.

I wish also to thank the following. The Physical Society of England, for permission to quote from a paper by J. Guild published in the *Transactions of the Optical Society*, Vol. xxi, 1929–30. The Royal Aëronautical Society of England, for permission to quote from a paper by Sir Horace Darwin published in *The Aëronautical Journal* for July, 1913. The University Press, Cambridge, for permission to quote from *Experimental Psychology* by C. S. Myers. Messrs. Chapman and Hall, Ltd., London, for permission to quote from *Aircraft Instruments* by J. C. Stewart.

This book would not have been written but for the encouragement of my father, Professor A. N. Whitehead; his work, in a very different field, has been the main influence in determining my own standpoint. Finally, I owe much to Mr. R. W. Cheshire, Senior Scientific Officer to the British Admiralty, with whom I had the good fortune to be associated for several years, and whose unfailing help in his double capacity of guide and colleague has left me deeply in his debt.

THOMAS NORTH WHITEHEAD

Harvard School of Business
Administration
Harvard University
November, 1933

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THE DESIGN AND USE OF INSTRUMENTS AND ACCURATE MECHANISM

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CHAPTER I

INSTRUMENTS AND THEIR PARTS

INSTRUMENTS AND MACHINES

The distinction between an instrument and any other piece of mechanism is not very definite. Broadly speaking, instruments either assist in the acquisition of knowledge, or else their purpose is to control some other mechanism. Chemical balances and microscopes are instances of the former, whilst thermostats illustrate the latter. As a class, instruments are small compared to machines and structures; they usually do little or no external work, and questions of safety do not often arise. The first requisite of an instrument is accuracy; other things being equal, the best instrument is the most accurate instrument; and instrument design should be based on an analysis of instrumental errors.

From the standpoint of design, the difference between an instrument and other mechanism is almost entirely one of emphasis. In both cases the same considerations apply, but in very different proportions. This is partly due to the difference in scale, and even more to the peculiar purpose for which instruments are intended, resulting in an unusual demand for accuracy. It frequently results that the component parts of an instrument must be made to fine limits; but this is not the type of accuracy referred to, and would not distinguish an instrument from many other machines. In the case of many machines, such as an automobile, no particular accuracy of final action is demanded, and close limits of manufacture relate themselves to such considerations as wear, vibration, thermal and mechanical efficiency, etc. When fine manufacture is related to the precision of the final action, to that extent is a machine approximating to an instrument. The engine of an automobile is coupled to the differential of the back axle by two universal joints and a driving shaft, but the precise angular relations existing between the crank shaft and the differential shaft is of little moment; apart from considerations of wear and a possibility of jerking the passengers, considerable play in these universal joints would not interfere with the performance of the vehicle. In an instrument the conditions are quite different, for its requirements essentially involve accuracy in the relative motions and positions of the component parts.

An instrument can be regarded as a chain of related parts, stretching from some object to an observer—thus a balance connects the observer to some object whose mass is sought—and the accuracy of an instrument, as a means for conveying information to an observer about an object, is directly dependent on the maintenance of exact relations between the component parts. The same general considerations apply to instruments whose purpose is to control other mechanism.

Thus, for the present purpose, *an instrument may be defined as any mechanism whose function is directly dependent on the accuracy with which the component parts achieve their required relationships*. This definition will include a number of mechanisms not usually classed as instruments, but the considerations involved in their design will always be typical of the instrument class.

ELEMENTS AND LINKS

Thus, most problems peculiar to instrument design do not tend to relate to individual parts, such as pistons and connection rods, so much as to small assemblies, involving the relation of one part to another. If an instrument contained a train of gears, the most important problem would probably be the possible backlash, not because of noise or wear, but because backlash leads to an uncertainty as to the angular relation existing between the driving and driven members. For this reason it is sometimes convenient to break down instruments into elementary assemblies, containing at least two parts, in order to study the relation between them. Such a small assembly will be called an *Element;* and the simple parts (usually two) composing an element will be referred to as *Links*.

The important point about an element is the relation subsisting between its links; for on these relations usually depends the accuracy of the instrument. Two spur wheels in mesh form a very simple example of an element, the two wheels taken separately being the links. In some instances the links are not separate parts in any obvious sense. Thus, if the torsional strain of a shaft is being considered, it is the two ends that represent the links; the element being these plus their relationship.

Thus an element is the smallest unit which retains the relationship between parts, given the scale of the examination; and if a change in a link itself is found to be disturbing its element (i.e., altering the relationship) this indicates that a smaller element consisting of the original link (now considered as two links related) should be considered.

MAINTAINING AND RESTORING AGENCIES

When two parts, comprising a simple assembly or element, are found to be in some persisting relation to each other, positive agencies must be present to produce this continuity; these will be called *preserving agencies*. Thus two meshed wheels retain certain angular relationships in virtue of forces which arise due to the presence of the teeth.

Preserving agencies are of two types: *maintaining agencies* and *restoring agencies*. In either case a departure from the given relation is opposed by a force. In the case of a *maintaining agency* the actual relation in being is more or less efficiently maintained; but no force exists to restore a previous relation which may have been disturbed. Thus, a weight W rests on a horizontal plane and the coefficient of friction (or more accurately stiction) between the two is μ. Then any attempt to disturb the relation between W and the plane, by causing a horizontal force to act on the former, will be opposed by an equal maintaining force, up to the limiting value μW. Above this value the relation will be disturbed, and the same maintaining agency will tend to preserve the new relation. Friction clamps are maintaining agencies, and, indeed, provide by far the most frequent examples, although other means for maintaining elements are not unknown. A spindle, intermittently driven, may be required to rotate at a sensibly uniform speed with respect to its housing. This element (spindle, housing, and the relation described) can be approximately maintained by mounting a flywheel on the spindle. Here, it is the inertia of the wheel which is the maintaining agent, and as before the inertia will oppose any change of the element's relation, but has no tendency to restore the relation once the latter is disturbed.

A *restoring agency* differs from a maintaining agency by resisting any disturbance from a predetermined relation; and in the absence of any disturbing, or maintaining forces, it restores an element to its original relation. In the case of a plumb line the element consists of the line, the earth, and the required relationship (i.e., that the line should give the direction of the earth's gravitational pull). The plumb line is liable to gross disturbances; but, on removing the disturbing causes, the element is eventually restored by means of the gravitational pull on the bob, which force is the restoring agency in this case. A spring loaded safety valve is another instance of an element with a restoring agency (the spring) which returns the valve to its seat when the disturbing cause (excessive pressure) is absent. In many cases an element is disturbed by straining some member of the mechanism; and the restoring agency is the force exerted by that member in its effort to relieve the strain, acting on the other links of the element.

ERRORS AND ELEMENTS

The errors of instruments are of three main types which correspond to three ways in which elements can be at fault.

1. False Element

The links in an element may not have the desired relation; this leads to *systematic errors*.

When the two arms of a balance are of unequal length, this constitutes a false element. Here the relation between the two arms is not the desired one, and the result is a persistent or systematic error in the behavior of the balance.

Systematic errors and false elements are considered in Chapter II.

2. Disturbed Element

The element, preserved by restoring agencies, may be disturbed by chance causes from without; then the element, for the time being, is not functioning as intended. This leads to *short period errors*. Vibration is a common example of a disturbing cause which leads to short period errors in otherwise good elements. These errors are often amenable to partial correction by mathematical procedure; they are sometimes referred to as "errors of observation" in the applied theory of probability. This is sometimes a misnomer and, in any case, short period errors have no exclusive claim to this title.

There is not much point in describing an element as "disturbed" if it is only preserved by maintaining agencies (as distinct from restoring agencies), for when such an element is disturbed it never restores itself and ceases to be the same element, because the old relation is completely lost. It is simpler to regard a maintained element as "incomplete" (see below) when it proves liable to disturbance.

As a matter of fact, most maintained elements also possess restoring agencies which act within narrow limits. Thus, two members, fastened together by a friction clamp, are maintained in the sense that if the clamp slips the original relation is completely lost; but, even supposing the clamp to grip, a small disturbance may well be possible through the elasticity of the materials involved, and, provided no slipping occurs, the original element is restored as soon as the disturbing cause ceases. In such a case the element possesses restoring agencies capable of handling small disturbances, but in the presence of larger disturbances the element has no powers of restoration.

Chapter III very briefly discusses the relevant mathematical theory, whilst Chapter IV deals with its application to short period errors and disturbed elements.

3. *Incomplete Element*

An element is incomplete when the relations between its links are not uniquely indicated by either maintaining or restoring agencies.

Thus, in the presence of backlash, the relation between two meshed wheels is ambiguous within limits. That is, this element is incomplete as regards the relation involved. This leads to *erratic errors* and is dealt with in Chapter V.

Erratic errors are not unlike short period errors except that they are far more difficult to discount by subsequent mathematical procedure. For no exact relationship is indicated (by preserving agencies) around which fluctuations can be said to be taking place.

SUMMARY OF THIS CHAPTER

This book is concerned with that type of mechanism whose function is directly dependent on the accuracy with which the component parts achieve their required relationships.

This requirement is particularly characteristic of instruments but also typifies other precise mechanisms.

The various errors to which such mechanism may be liable can be collected into three groups, each group resulting from a characteristic type of fault in one or more elements.

Thus:

Fault in Element	*Resulting Error*
1. False element	Systematic error
2. Disturbed element	Short period error
3. Incomplete element	Erratic error

Part I discusses these three sorts of faulty elements and their resulting errors; whilst in Part II a few general principles of design are considered in relation to these defects.

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PART I

THE THEORY OF ERRORS

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CHAPTER II

SYSTEMATIC ERRORS (FALSE ELEMENTS)

The simplest case of a systematic error is that of an instrument which possesses a constant zero error. For example, an engineer's screw micrometer gauge, Fig. 1, is often sensibly accurate (within the limits demanded of it) except for a constant zero error.

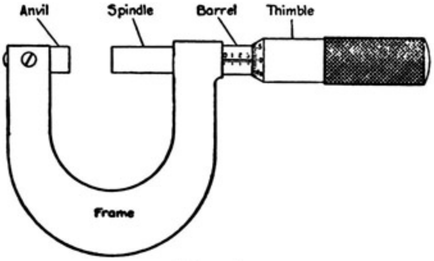

FIG. 1.

Let this error be a; then, if a length L be measured the fractional error,

$$\eta = \frac{a}{L}.$$

Suppose that in addition the micrometer's screw has a uniformly incorrect pitch, the actual error,

$$\epsilon = a + bL;$$

and

$$\eta = \frac{a}{L} + b.$$

If $a=0$, then η is a constant and ϵ varies with L, but in the presence of a and b the instrument cannot properly be said to have any one definite accuracy, either actual or fractional, apart from the quantity measured. Of course, the greatest actual or percentage error of the instrument can always be stated. Thus, in the present case,

$$\epsilon_{\text{max.}} = a + b(L_{\text{max.}}),$$

and

$$\eta_{\text{max.}} = \frac{a}{0} + b = \infty.$$

But this last statement seems hardly fair to the instrument, and, in any case, conveys no information. For remarkably few instruments are either ϵ or η constant, and the only answer to the above question is to give their value in terms of the relevant variables.

These variables are not necessarily connected with the quantity measured. Thus, in the absence of a zero error, if the incorrect pitch of the micrometer screw were due to the instrument being used at a temperature for which it was not intended, the errors could be expressed as:

$$\epsilon = C(t_2 - t_1)L,$$

and

$$\eta = C(t_2 - t_1)$$

where t_1 represents that temperature at which the screw is correct and t_2 the temperature in use, C stands for the coefficient of expansion of the metal, supposed constant for moderate changes of temperature.

Both the zero error a and the scale error bL are the outcome of false elements. In the first instance, the false element can be regarded as either that containing the zero mark on the thimble and the fiducial line on the barrel, or the element containing the anvil and the frame. Since the former is usually incapable of adjustment, whilst the latter (in some makes) can be adjusted after slacking back the screw S, it is more useful to consider this element as false. The scale error results from the fact that the element containing the screw is false, i.e., the various parts of the screw thread have not the correct relations with each other. Both these errors could be eliminated by adjustment, the adjustment of the latter being an alteration of temperature. If the numerical values of a and b are known they can be discounted by calculation, whilst if unknown, by a suitable method of measurement.

Thus, a differential measurement would result in two readings R_1 and R_2 burdened with errors,

$$a+bR_1, \text{ and } a+bR_2, \text{ respectively.}$$

The error of this differential measurement

$$\begin{aligned}\epsilon &= (a+bR_2)-(a+bR_1)\\ &= b(R_2-R_1)\\ &= bL.\end{aligned}$$

A simple measurement by substitution consists in measuring a known length L_1 having approximately the same value as the unknown. If the known length be measured as R_1 and the unknown as R_2, the unknown length L will be evaluated as

$$L=L_1+(R_2-R_1),$$

and the error is $b(R_2-R_1)$. Since R_2 and R_1 are nearly equal by hypothesis, this error will probably not be significant, but a differential measurement by substitution would clear both R_1 and R_2 from zero error, and it follows that in the absence of errors other than a and b,

$$R_1 : L_1 = R_2 : L,$$

or

$$L=\frac{R_2}{R_1}\cdot L_1$$

and this measurement is free from errors a and b. In practice, it is rarely worth being too meticulous in this type of measurement as many other causes of error probably exist, e.g., cyclical errors in the screw, possible passivity, etc.; and to pursue any one possible error too relentlessly, unless the others are also controlled, only leads to a false sense of security. No general rules can be laid down, but my experience is that in the case of mechanical instruments (as distinct from electrical) if the overall error of a measurement has been reduced to 10% of the average errors of its observations by a choice of method the observer has been singularly fortunate; in a great many instances, a reduction to 30% of the errors of observations is all that can be counted on. However, the value of a suitable choice of method varies enormously from one case to another, and is obviously greatest where accuracy of the observations is demonstrably depressed by some one or two causes which can be controlled by method.

The systematic errors of three typical instruments will now be considered; and, since the object of this chapter is not primarily to investigate the instruments, but to discuss a certain type of error, no attempt will be made to cover all the errors to which these instruments may be liable.

Hilger Photomeasuring Micrometer

Fig. 2 gives a diagrammatic view of this instrument designed and made by Messrs. Adam Hilger, whilst Figs. 62 and 65 are photographs of the actual instrument. This is an instrument of great accuracy and will measure lengths up to 150 mm., with a least count of 1 μ[1] by vernier; or, as some observers prefer, by estimation from a fiducial line.

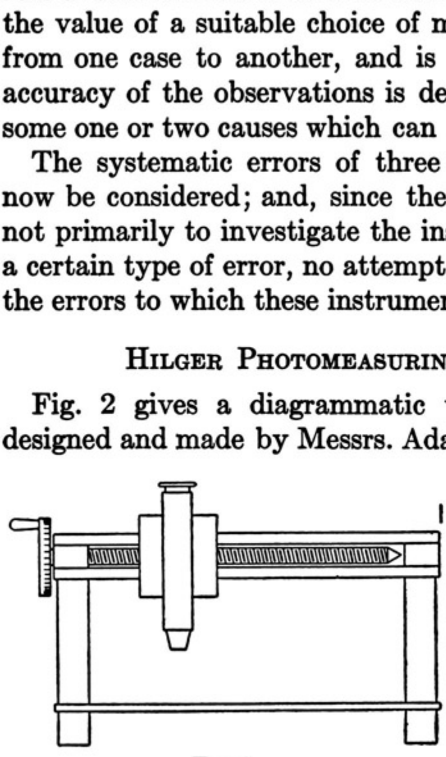

FIG. 2.

The length to be measured, usually the distance between two marks, is placed on the stage below the microscope and parallel to its direction of travel. This adjustment can in practice be performed in the following way: The microscope carries a graticule in the focal plane of the objective which may consist of two fine lines crossing at right angles in the centre of the field. One line lies along the direction of travel and the other at right angles. The length to be measured is placed so that, as the microscope is run along its slide, the point of intersection of the graticule lines passes successively over the two points whose distance apart is to be measured. There are no adjustments on the stage to assist this operation. Attached to the microscope mount is a nut engaging a very accurate screw with a pitch of 1 mm. This screw performs the double function of moving the microscope along its guide and of registering the amount of this movement. One end of the screw carries a handle and a drum, the periphery of which is divided into a hundred parts, each division being equivalent to 10 μ; whilst the frame carries a vernier reading to a single μ. Whole millimetres are read off a millimetre scale, attached to the frame, by the help of an engraved line on the microscope mount. To obtain the measurement, observations are made at both ends of the length and the difference of their readings is the required value of the unknown length.

---

[1] The Greek letter μ stands for the word micron, meaning thousandth, and is used to signify a one-thousandth part of a millimetre. Similarly, $\mu\mu = 10^{-6}$ mm.

The accuracy of this instrument depends on the fulfillment of a number of conditions, and, without attempting to consider them all, it will be instructive to take a few of the possible causes of systematic error and derive an expression for the error of observation in terms of these causes.

Three quantities are involved: first, the length to be measured, L; next, the distance through which the microscope travels when measuring L—this distance will be called T; and finally, the distance, R, read off the graduated drum and scale.

The required condition for accuracy is that

$$R=L$$

but R only records T, and not primarily L, so a second condition is that

$$T = L;$$

or finally

$$R = T = L.$$

Four possible faults in the manufacture and adjustment of the instrument will be considered, all of which lead to systematic errors (if any), and an expression will be obtained giving ϵ and η; that is, the actual and fractional amount by which R fails to correspond with L. The length to be measured is supposed to be the distance between two parallel lines on a photographic plate, and Fig. 3 shows the instrument in adjustment.

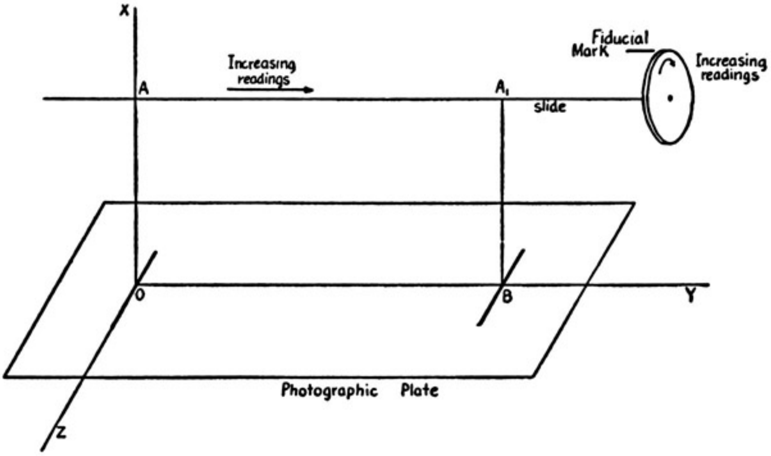

FIG. 3.

The two lines O and B, whose distance apart is L, are normal to the path OB of the microscope as it travels from O to B. AO and A_1B represent the optical axis of the microscope when set on O and B, respectively. $AA_1 = T_1$, the microscope's travel on its slide, and all angles are right angles.

Fig. 4 shows the same instrument with three possible causes of error.

First; the plate is rotated in its own plane through an angle α, so that the length measured, OB', is no longer a true measure of the distance between the photographic lines; this is a rotation about the X-axis.

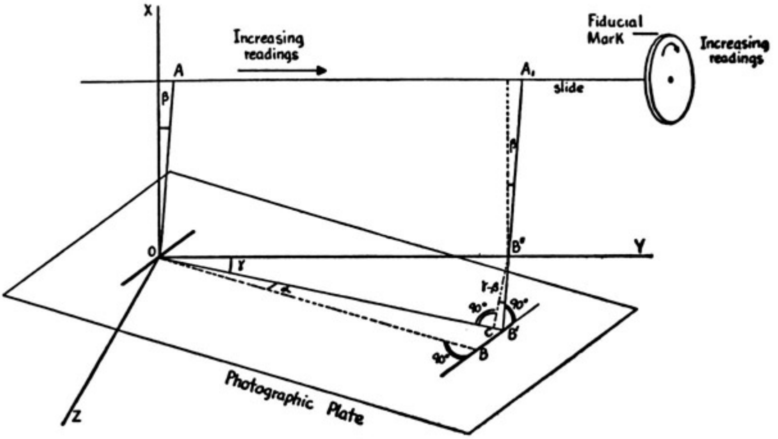

FIG. 4.

Secondly; the plate is rotated about the Z-axis through the angle γ; this results in one end of the plate being further from the slide than the other end.

Thirdly; the microscope is rotated about the Z-axis through the angle β; that is to say, the optical axis makes an angle β with the slide and in the direction of travel, so continuing to lie in the plane $Z=O$.

The first two causes of error represent a false element (microscope slide, unknown length); the restoring agency in this case is the observer's visual acuity and judgment when setting up the photographic plate; this applies to the angles α and γ, since the latter is controlled by noting that the focus of the microscope is the same when set on O and B; though there is no adjustment provided if this should not be so.

Another false element (optical axis, slide) accounts for the third cause of error. The optical axis is determined by the intersection of the graticule lines and the objective of the microscope, and the former has an adjustment.

In the figure draw $B''C$ to meet OB' normally at C.

$$T = AA_1 = OB'',$$
$$OB = OB' \operatorname{Cos} \alpha,$$

and

$$OB' - CB' = OB'' \operatorname{Cos} \gamma;$$

hence

$$\begin{aligned} OB' &= OB'' \operatorname{Cos} \gamma + CB'' \operatorname{Tan} (\gamma - \beta) \\ &= OB'' [\operatorname{Cos} \gamma + \operatorname{Sin} \gamma \operatorname{Tan} (\gamma - \beta)]; \\ L &= T \operatorname{Cos} \alpha [\operatorname{Cos} \gamma + \operatorname{Sin} \gamma \operatorname{Tan} (\gamma - \beta)]. \end{aligned}$$

The microscope is caused to travel by the rotation of the lead screw carrying the graduated drum, and one revolution of this screw advances the microscope one millimetre, which is also the unit of length for this instrument. Hence, assuming an accurate pitch and no other sources of error, T can be expressed as a difference between the angular positions of the zero mark of the drum when the microscope is first at A and then A_1; namely, θ_1 and θ_2.

So measuring the angles in radians

$$T = \frac{\theta_2 - \theta_1}{2\pi} \text{ millimetres}$$

and

$$L = \frac{\theta_2 - \theta_1}{2\pi} \operatorname{Cos} \alpha [\operatorname{Cos} \gamma + \operatorname{Sin} \gamma \operatorname{Tan} (\gamma - \beta)] \text{ mm.} \qquad \text{I}$$

This expresses the length L in terms of the drum reading.

Had the instrument been accurate, this length would have been recorded as $\frac{\theta_2 - \theta_1}{2\pi}$ millimetres. Hence

$$\epsilon=\frac{\theta_2-\theta_1}{2\pi}\text{ Cos }\alpha\,[\text{Cos }\gamma+\text{Sin }\gamma\text{ Tan }(\gamma-\beta)]-\frac{\theta_2-\theta_1}{2\pi}$$

$$=\frac{\theta_2-\theta_1}{2\pi}\,\{\text{Cos }\alpha\,[\text{Cos }\gamma+\text{Sin }\gamma\text{ Tan }(\gamma-\beta)]-1\}\text{ mm.}\qquad\text{II}$$

and

$$\eta=\text{Cos }\alpha[\text{Cos }\gamma+\text{Sin }\gamma\text{ Tan }(\gamma-\beta)]-1.\qquad\text{III}$$

This last expression is a ratio and has no dimensions. These errors are positive when

$$R>L.$$

Equation II shows that the absolute value of the error varies directly as the length to be measured, given any particular values of α, β, and γ; whilst it follows from this, and is plainly shown in Equation III, that the fractional error is a constant for all values of L.

No instrument can be perfect, and all intended adjustments will carry some error, large or small, depending partly on the accuracy of workmanship and partly on the relation between the size of the maladjustment and the magnitude of the resultant error. This last is, to a certain extent, in the hands of the designer who, by skillful design, can often arrange that the final reading shall be comparatively insensitive to some given adjustment. In any case, some one or two features of the instrument will be found to be the bottle neck preventing any substantial improvement; whilst errors due to other adjustments can be reduced to negligible proportions by ordinary care in design and workshop. So errors fall into two classes, those that have only to be recognized in order that they may be effectively eliminated; and those errors that determine the accuracy of the instrument. Sometimes, as stated above, some errors can be removed from the second class by skillful design, but never all; otherwise an instrument would be capable of indefinite improvement.

It follows that the first class demands a given degree of workmanship, and then these errors vanish, or at least become so small that better workmanship would be wasted; the second class can never be so small but that their reduction will improve the performance of the instrument to much the same extent.

Now, one of the uses to which Formulae II and III can be put is to determine whether the errors resulting from α, β, γ, represent the bottle neck to the instrument or can be reduced to negligible quantities.

Consider Formula III, which can be written

$$\eta=[\text{Cos }\alpha\text{ Cos }\gamma-1]+[\text{Cos }\alpha\text{ Sin }\gamma\text{ Tan }(\gamma-\beta)]. \quad \text{IIIa}$$

α, β, and γ are all small angles and may be positive or negative (they are all drawn positive in the figure); hence, their cosines will all be positive; whilst their sines and tangents may be positive or negative, following the signs of the angles.

It follows that the term Cos α Cos γ must be positive and slightly less than unity, whilst the term in the second bracket may be positive or negative.

The expression in the first bracket consists of the difference between the product of two cosine terms and unity, and since the cosine of a small angle is very nearly one, this error does not appear serious.

The second expression, the product of a sine and tangent, is obviously the more important since these functions change rapidly with small angles. The term Cos α may be ignored in this expression as only effecting a very small percentage change in Sin γ Tan $(\gamma-\beta)$.

On the other hand, these angles α, β, γ are not likely to be of the same magnitude; for β is a permanent adjustment provided with a suitable adjustment and, within limits, can be correctly set once for all. α is entirely in the hands of the observer, and on occasion is not easy to reduce to very small limits. γ falls midway between the other two; the manufacturer is responsible that the stage is sensibly parallel to the microscope's slide, but the user must bear the responsibility for seeing that the object, whose length is to be measured, is lying flat on the stage, or at least that the two points of measurement are equally distant from the microscope.

This presents difficulty in the case of a photographic plate which is not infrequently anything but plane. Thus we may expect

$$\alpha > \gamma > \beta.$$

But if $\gamma > \beta$ the expression

$$\operatorname{Cos} \alpha \operatorname{Sin} \gamma \operatorname{Tan} (\gamma - \beta),$$

cannot be negative; for Tan $(\gamma - \beta)$ will take the sign of γ, and Sin γ Tan $(\gamma - \beta)$ will then be positive whether γ is positive or negative; whilst Cos α is necessarily positive. Hence, the second term, if large, will be positive.

Assume that the length L is about 100 mm. and that

$$\begin{aligned} \alpha &= +30' \\ \beta &= -5' \\ \gamma &= +10'. \end{aligned}$$

These limits present no difficulties. Then the expression in the first bracket $= -0.00004$, and that in the second $= +0.00013$.

$$\therefore \eta = +0.00009.$$

So ϵ, the absolute error, would be 9 μ in a length of 100 mm.

This is not good in an instrument reading to single μ and requires some investigation.

The error of -0.00004 in the first bracket is almost entirely due to α, since Cos $10' = 1$ to at least seven significant figures.

This suggests that the observer must go to some trouble to get his work sufficiently parallel to the travel of the microscope, and that perhaps some fitting on the stage might be supplied to help him. He should not let α exceed about 10′ in this case. In the second bracket β and γ are both significant, γ being the more important. γ is again the business of the observer who could evidently be helped by a levelling adjustment on the stage. β falls to the manufacturer in the first instance, though an observer's graticule adjustment is provided.

It will be seen from this that whilst these adjustments need not prove the limiting factors in the instrument's performance, they cannot be ignored and require careful consideration on the parts of the designer and observer. In the case just considered, the expressions in the two brackets (Equation IIIa) tend to be of opposite sign and so compensate each other; if the measurement had been between two points, not lines, the opposite would have been the case, but then it would be far more easy for the observer to reduce the angle α.

Consider the errors due to α, β, γ, taken separately. From Equation III:

If α exists alone,

$$\eta = \operatorname{Cos} \alpha - 1.$$

If β exists alone,

$$\eta = 0.$$

If γ exists alone,

$$\eta = \operatorname{Cos} \gamma + \operatorname{Sin} \gamma \operatorname{Tan} \gamma - 1.$$

The sum of these separate errors is

$$\operatorname{Cos} \alpha + \operatorname{Cos} \gamma + \operatorname{Sin} \gamma \operatorname{Tan} \gamma - 2,$$

which is not the equivalent of Equation III, and, giving α, β, γ, the same values as above, the sum of these errors amounts to 5 μ in 100 mm., or only just over half the error already obtained. This illustrates the fallacy of supposing that the error from a number of causes is, in general, the same as the sum of the errors of each cause taken singly. This would only be true if each error were independent of every other one, a fact not always easy to be sure of, and very rarely true when the errors are dependent on the various departures of any one element. In these circumstances the natural inclination to avoid three-dimensional geometry by taking the problem in successive stages should be resisted.

It is interesting to notice that β, taken alone, produces no error; although in combination with γ its influence is by no means negligible.

One further source of error will now be considered. It has been assumed that the rotation of the screw in travelling the microscope, i.e., $\frac{\theta_2-\theta_1}{2\pi}$, would be accurately registered by the recording drum; that is, that the element (screw, drum) is true. If the drum were eccentrically engraved or mounted, an error would occur from this cause.

In Fig. 5, $F'ZF$ represents the drum, the periphery of which is divided into a hundred parts; each division being the equivalent of $\frac{1}{100}$ mm. travel of the microscope, or 10 μ. These divisions are supposed to be engraved from the centre O, whilst C is the centre of rotation of the drum. O and C lie on the diameter FF'. Z represents the zero division; and D the reading opposite the fiducial mark, D' being the reading that should have been at D had the drum been concentrically engraved. The figure, with respect to Fig. 3, is an elevation seen from the left. The instrument in question actually carries a vernier, but it is quite usual to use the first vernier division as a fiducial mark and

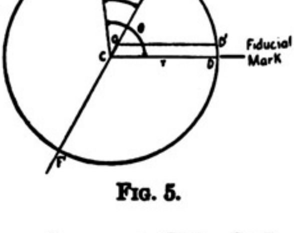

FIG. 5.

obtain the last significant figure by estimation. The vernier will be ignored.

Angles are measured from the radius passing through the zero CZ, and the positive direction is taken as clockwise.

The angle δ between the zero mark and FF' can evidently have any value between 0 and 2π and is a constant of the instrument; whilst the angle θ between CZ and the fiducial mark may be anything between 0 and $2\pi.n$, where n is positive and depends on the limiting travel of the microscope. The eccentricity of the graduations is CO, and $\frac{CO}{r}$ will be denoted by c, where r is the radius of the drum.

The error in recording any angle θ is DD'

$$= c \operatorname{Cos}(\theta - \delta) \text{ very nearly.}$$

Hence the actual reading registered for any angle θ

$$= \theta + c \operatorname{Cos}(\theta - \delta).$$

And the recorded travel of the microscope becomes

$$\frac{[\theta_2 - \theta_1] + c[\operatorname{Cos}(\theta_2 - \delta) - \operatorname{Cos}(\theta_1 - \delta)]}{2\pi} \text{ mm.} \qquad \text{IV}$$

Substituting this expression in Equation I in place of $\frac{\theta_2 - \theta_1}{2\pi}$ which represents the actual rotation of the leading screw, we obtain

$$L = \frac{\{[\theta_2 - \theta_1] + c[\operatorname{Cos}(\theta_2 - \delta) - \operatorname{Cos}(\theta_1 - \delta)]\} \operatorname{Cos}\alpha[\operatorname{Cos}\gamma + \operatorname{Sin}\gamma \operatorname{Tan}(\gamma - \beta)]}{2\pi} \text{ mm.} \qquad \text{V}$$

as the value of L in terms of the actual reading and the previously considered angles α, β, γ.

From this we obtain the error of the recorded length R, namely:

$$\epsilon = \frac{\{[\theta_2 - \theta_1] + c[\operatorname{Cos}(\theta_2 - \delta) - \operatorname{Cos}(\theta_1 - \delta)]\} \operatorname{Cos}\alpha[\operatorname{Cos}\gamma + \operatorname{Sin}\gamma \operatorname{Tan}(\gamma - \beta)] - [\theta_2 - \theta_1]}{2\pi}$$

$$= \frac{[\theta_2 - \theta_1]\{\operatorname{Cos}\alpha[\operatorname{Cos}\gamma + \operatorname{Sin}\gamma \operatorname{Tan}(\gamma - \beta)] - 1\} + c[\operatorname{Cos}(\theta_2 - \delta) - \operatorname{Cos}(\theta_1 - \delta)] \operatorname{Cos}\alpha[\operatorname{Cos}\gamma + \operatorname{Sin}\gamma \operatorname{Tan}(\gamma - \beta)]}{2\pi} \text{ mm.} \qquad \text{VI}$$

And

$$\eta=\frac{\epsilon}{\frac{\theta_2-\theta_1}{2\pi}}$$
$$=\{\text{Cos}\ \alpha[\text{Cos}\ \gamma+\text{Sin}\ \gamma\ \text{Tan}\ (\gamma-\beta)]-1\}+$$
$$\frac{c[\text{Cos}\ (\theta_2-\delta)-\text{Cos}\ (\theta_1-\delta)]\ \text{Cos}\ \alpha[\text{Cos}\ \gamma+\text{Sin}\ \gamma\ \text{Tan}\ (\gamma-\beta)]}{\theta_2-\theta_1} \qquad \text{VII}$$

Equation VI is composed of the sum of two expressions of which the first is

$$\frac{\theta_2-\theta_1}{2\pi}\{\text{Cos}\ \alpha[\text{Cos}\ \gamma+\text{Sin}\ \gamma\ \text{Tan}\ (\gamma-\beta)]-1\}.$$

This is identical with the expression obtained in Equation II for the recording error due to α, β, and γ. We will call this $\epsilon_{\alpha\beta\gamma}$, meaning that error due to these angles, *assuming no other errors to exist.* $\eta_{\alpha\beta\gamma}$ will carry a similar meaning.

The second expression in Equation VI, namely:

$$c[\text{Cos}\ (\theta_2-\delta)-\text{Cos}\ (\theta_1-\delta)]\ \text{Cos}\ \alpha[\text{Cos}\ \gamma+\ \text{Sin}\ \gamma\ \text{Tan}\ (\gamma-\beta)]$$

consists of the product of two terms, the second of which has already been considered in some detail.

It was found that

$$\text{Cos}\ \alpha[\text{Cos}\ \gamma+\ \text{Sin}\ \gamma\ \text{Tan}\ (\gamma-\beta)]-1,$$

the fractional error due to α, β, and γ, only differed very slightly from zero. In fact, in a numerical case where α, β, and γ were found to be intolerably large, η only amounted to 9 in 100 mm. travel; or in other words,

$$\text{Cos}\ \alpha[\text{Cos}\ \gamma+\ \text{Sin}\ \gamma\ \text{Tan}\ (\gamma-\beta)]$$

only differed from unity by about one part in ten thousand or 0.01%. Consequently, this term only affects the value of

$$c[\text{Cos}\ (\theta_2-\delta)-\text{Cos}\ (\theta_1-\delta)]$$

to the extent of 0.01% and can safely be ignored.

So, to a high order of accuracy, the recording error, when measuring L in the presence of α, β, γ, and the eccentricity c, is

$$\epsilon = \epsilon_{\alpha\beta\gamma} + \frac{c[\mathrm{Cos}\ (\theta_2 - \delta) - \mathrm{Cos}\ (\theta_1 - \delta)]}{2\pi}\ \mathrm{mm.} \qquad \mathrm{VIa}$$

and

$$\eta = \eta_{\alpha\beta\gamma} + \frac{c[\mathrm{Cos}\ (\theta_2 - \delta) - \mathrm{Cos}\ (\theta_1 - \delta)].}{\theta_2 - \theta_1} \qquad \mathrm{VIIa}$$

These errors are interesting as giving an instance of two different sources of error that can be considered quite separately, in that the final error is the sum of the two partial errors, their interaction being very slight.

So confining attention to the partial error due to the presence of c,

$$\epsilon_c = \frac{c[\mathrm{Cos}\ (\theta_2 - \delta) - \mathrm{Cos}\ (\theta_1 - \delta)]}{2\pi}\ \mathrm{mm.} \qquad \mathrm{VIII}$$

and

$$\eta_c = \frac{c[\mathrm{Cos}\ (\theta_2 - \delta) - \mathrm{Cos}\ (\theta_1 - \delta)]}{\theta_2 - \theta_1}. \qquad \mathrm{IX}$$

ϵ_c is seen to vary directly as c and as the difference of two cosine terms. Now δ may have any value from 0 to 2π; and θ can be anything from 0 to $2\pi.n$, where n is a fairly large positive number. Again δ is a constant of the instrument, whilst θ_1 and θ_2 are quite independent variables; hence, both cosine terms may be of either sign, and both may simultaneously have their maximum value of unity.

$$\text{Hence } \epsilon_c \text{ maximum} = \pm \frac{c}{\pi}\ \mathrm{mm.} \qquad \mathrm{X}$$

$$\text{Whilst } \eta_c \text{ maximum} = \pm \frac{2c}{\theta_2 - \theta_1}. \qquad \mathrm{XI}$$

In the case of periodic errors such as ϵ_c and η_c their maxima are often of far more importance than the particular nature of their fluctuations, which latter can in any case only be used by applying a tedious correction to each observation.

Particularly, it is the maxima of a periodic error which are important to the designer, who will wish to eliminate it by improved design and workmanship. It will be seen that ϵ_c max. has two values $+$ *and* $-$ $\frac{c}{\pi}$ which are constants of the instrument. η_c max. naturally varies inversely as the length measured, and also has two numerically equal values for any given value of $\theta_2 - \theta_1$.

Thus, with these sources of error the instrument cannot strictly be said to possess any definite percentage or actual error; both depend on the length to be measured.

Since this instrument records to 1 μ, and in certain circumstances will give values to less than this, we will assume that

$$\epsilon_c \text{ max.} \overset{=}{<} 0.3\ \mu.$$

From X,

$$\epsilon \text{ max. in mm.} = \frac{c}{\pi}.$$

So

$$0.0003 = \frac{c}{\pi}.$$

$$c = 0.001.$$

But $c = \frac{CO}{r}$ (Fig. 5), and in this instrument $r = 60$ mm. approximately.

Hence

$$CO_{\text{max.}} = cr = 0.001 \times 60$$

$$= 0.06 \text{ mm.}$$

or about two-thousandths of an inch.

This is the maximum eccentricity allowable on the drum if errors from this source are never to exceed 0.3 μ. The drum must clearly be carefully made, but if its centre is finished machined, and then used as the location when engraving, the centring error should be less than this.

The errors we have so far discussed have not proved to be the limiting factors to the accuracy of the comparator, though the angles α, β, and γ evidently require more care than they often get in the case of measurements of comparatively long length. Probably the most serious errors connected with an instrument of this nature are connected with the truth of the leading screw and nut; some of these errors are systematic whilst others are erratic and the remainder short period. The most obvious systematic error is an error of scale, due for instance to the screw being cut at one temperature and used at another; or more precisely, due to the master screw on the lathe being correct at one temperature, cutting the instrument's screw at a second temperature, and the use of the instrument at a third. If the master and the instrument's screws have different temperature coefficients, the latter will be correct at some fourth temperature which is best determined by experiment.

The temperature coefficient of a steel screw may be expected to be in the neighborhood of 10×10^{-6} per °C., unless invar steel is used. This results in

$$\eta = 0.000001 \text{ per } ^\circ\text{C.}$$

from this cause alone.

If we wish to measure 100 mm. correct to 1 μ, η from all causes must not exceed 0.00001.

In other words the correct temperature for the screw and the actual working temperature must be known to less than 1 °C., quite apart from any doubt as to its actual temperature coefficient.

Other systematic errors in the pitch take the form of recurring errors, which, if plotted against length, often approximate to a sine curve. A number of such errors can and do exist simultaneously, each error having its characteristic pitch. Fortunately the most marked pitch is usually that of the thread itself; that is to say, the worst of such errors generally recurs once for every turn of the screw thread, and, since the nut will cover a number of turns of the screw thread, it picks up an average movement which greatly minimizes this disturbance.

Erratic and short period errors are considered in another chapter, but it may be stated that these are largely due to possible lack of consistency between the movement of the microscope and the rotation of the screw. Thus, the nut may be free to shake on the screw, or the latter have end-play. Actually both elements are supplied with restoring agencies which indicate some definite relationship, but in the presence of friction between the microscope's saddle and slide it will be shown that these elements may behave as though they were incomplete. Another cause of erratic and short period errors is often to be found in the element connecting the graticule line with the object observed; the restoring agency is mental and is also considered later.

The most dangerous systematic errors in probable order of importance are perhaps:

1. The screw cut to the wrong scale; this will not be so gross but that at some possible temperature this error disappears and so can be expressed as a temperature error; but in practice the observer very rarely knows this temperature or the required coefficient.
2. The atmospheric conditions also affect the object being measured, and in the case of a photographic film this error is easily the most serious, humidity being a factor as well as temperature. Two films with a similar past history tend to change together.
3. The adjustment of the apparatus including the object to be measured, i.e., the angles α, β, and γ.

4. The cyclical errors of the screw, especially short period errors.

A suitable method of measurement and design can control these causes of error in varying degrees.

The measurement, however conducted, is essentially differential; hence, if any known length be measured, the "scale correction" for error No. 1 can be immediately found. This does not help to reduce the other three errors, although the second error, in the case of a material dependent on temperature alone, is effectively dealt with by recording its temperature and length, and repeating at another temperature if its coefficient of expansion is unknown. Unless the length is only required at room temperature the last operation raises a host of difficulties and requires a specially designed comparator.

In practice it is surprising how rarely an absolute determination of any quantity is ever required. When a rod and a circular hole are specified as having a given diameter, minus and plus a tolerance, it is because the one has to fit inside the other, and their *relation* is critical. Similarly in the case of most scientific measurement it is in reality some ratio or difference between two quantities that is required; often, though not always, it is possible to so arrange matters that this difference is measured more or less directly, or at least in ways burdened with the same errors, instead of measuring each independently and subtracting.

In the case of a photographic plate, it is often possible to photograph some standard length at the same time as the unknown. Thus a film, when used for micrometric work, is often pressed against a thick glass plate at the moment of exposure to insure the film lying flat in the focal plane. This plate has two or more fine parallel lines engraved on the surface contacting with the sensitive side of the film. The distance between these lines is accurately known, and the distance between their shadow-graphs on the negative is measured at the same time as the unknown length, thus automatically correcting for scale both in the lead screw of the comparator and in the plate itself.

The only way to control errors (3) and (4) in the absence of any positive knowledge as to their characters is to repeat the observations several times taking care to throw out and reset the adjustments between each repetition; this also ensures that slightly different parts of the lead screw are used on each occasion. This is discussed further in the next chapter.

An Analogous Problem

The following problem, which I met some years ago, illustrates the use that can be made of suitable methods of measurement to achieve adequate accuracy by the simplest means. A large number of photographs had been taken in pairs. Any one pair, plates A and B, were photographs of the same object, taken from the same position, with a time interval between them. The object of the experiment was to discover what change in dimensions, if any, had taken place in the object between the photographs A and B. The object was a plane surface with a number of distinguishing marks scattered over it, and, denoting the marks on A by the letters $a, b, c, d \cdots$, and on B by $a', b', c', d' \cdots$, the following quantities had to be evaluated: $ab-a'b'$, $bc-b'c'$, $cd-c'd'$, $\cdots$, and also a few cross lengths, such as $ac-a'c'$, etc.

The photographic lengths, ab, etc., varied from about 2 or 3 inches up to about 12 inches, and lay across the plates A and B in any direction, but these *two* plates had both been taken from the same position and it was known that the differences, $ab-a'b'$, etc., would not amount to more than about 0.2 or 0.3 of an inch at most. Every pair of plates, about a hundred, had to be measured in this way and the differences expressed with a mean error not greater than

0.0005 inch. The very large number of observations made it imperative that this accuracy should be achieved by repeating each observation twice only,[1] and as no comparator was available which could measure these lengths conveniently, having regard to the large photographic plates involved, an instrument had to be designed and made. Finally, since it is improbable that this instrument will ever be used again, cost was a consideration.

Fig. 6 shows the instrument in diagrammatic form. *A* and *B* are the two negatives, each placed in a hollow frame held in the turntable T_A and T_B. These turntables are capable of rotation in the slides S_A and S_B, S_A is capable of a limited horizontal travel between the guides G_A, whilst S_B slides in a direction at right angles, controlled by the guides G_B; S_A and S_B are carried on the main slide S_M which is itself carried by the frame *F* and is capable of travelling across the instrument as shown, controlled by

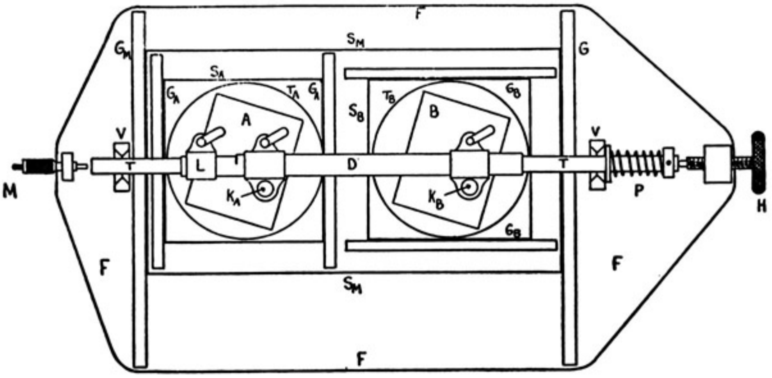

FIG. 6.

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[1] The object of duplicating each observation was not so much to reduce the mean error as to guard against a huge error, which would be shown by an unusual difference between the two readings, and also to insure against mistakes (as distinct from errors). The average accuracy and the "security" of a number of readings, meaning by the latter the probable absence of mistakes and "huge errors", are two very different things. This is mentioned again in the next chapter.

the guides G. This completes the movements of the plates with respect to the frame F. The observations are performed by means of the two microscopes K_A and K_B which are independently clamped to the sleeve D, the latter capable of sliding on the hollow tube T and being clamped thereto by L. T, which is plugged at each end, is constrained to slide in the guides V, and prevented from rotating (by means not shown); it is held in contact with the hand screw H by the spring P.

At the other end of the instrument the micrometer head M is held fast to the frame F, and measures the distance of the end face of the tube T from an arbitrary zero. M is a standard micrometer head reading to 0.0001 inch (2.5 μ).

The method of operation is as follows: The microscopes K_A and K_B are clamped to the sleeve D, such that their distance apart is roughly that between the centres of the turntables T_A and T_B. The two lengths to be compared, L_A and L_B, on plates A and B respectively, are brought into line and under the line joining the two microscopes by means of the plate adjustments already described, then D is slid along T until the graticule cross lines in K_A roughly coincide with one end of L_A; the corresponding end of L_B is then brought under K_B by means of the slide S_B. None of these adjustments are micrometric and after a few trials are very quickly performed. The actual measurement consists of four observations (and each was repeated once).

1. By means of the hand screw H, K_A is brought into exact register with one end of L_A, and a reading taken by means of M, say l_{A1}.
2. By means of the hand screw H, K_B is brought into exact register with the corresponding end of L_B, and a reading taken on M, l_{B1}. The clamp L is then loosened and D slid along the tube T until K_A is roughly in register with the other end of L_A, then D is clamped.

3. By means of H, K_A is brought into exact register with the second end of L_A, and a reading on M taken, l_{A2}.
4. Finally, by K, K_B is registered with the second end of L_B, the reading on M being l_{B2}.

From Fig. 7

$$\begin{aligned} L_B &= L_A - (l_{B1} - l_{A1}) + (l_{B2} - l_{A2}) \\ &= L_A + (l_{B2} - l_{B1}) - (l_{A2} - l_{A1}). \end{aligned}$$

Notice that neither L_A nor L_B have been evaluated although their difference has been found, and the tube T, on whose movements the whole measurement depends, has never moved more than a few tenths of an inch irrespective of the lengths compared. Since $l_{B1} - l_{A1}$ and $l_{B2} - l_{A2}$ very rarely exceeded a few hundredths of an inch, the parallelism of L_A and L_B was not critical.

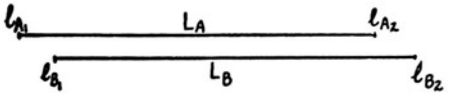

FIG. 7.

The measurement depends on three things:

1. The tube T must be straight and move parallel to itself.
2. The distance between the optical axes of the two microscopes, where they cut the plates, must remain invariable for the duration of the four observations. This implies that the axes shall be parallel or that the distance from objectives to plates should not change as the sleeve D is slid over T; the latter is necessary in any case as no microscope can be refocussed without risking a shift of its optical axis.
3. The micrometer M will introduce errors equal to its own proper errors.

All the other adjustments are not micrometric and they are only required to stay put for the duration of each complete measurement. In particular, no accuracy is demanded of the hand screw H which may be any device for moving T

and holding it for a few seconds. The only really severe requirement is in connection with the micrometer head M, and this can be bought for a small sum. All other parts can be made to ordinary engineering limits; nevertheless, the instrument very easily compares, though it does not measure, lengths of 12 inches with an error less than 0.0005 inch, or within 0.005%. Had the same comparison been made by direct measurements of L_A and L_B the cost of the lead screw and its mounting would have been very great, and far greater care would have had to be taken in order to get the unknown lengths parallel to the direction of travel of the microscope.

The relations in the various elements forming a measuring instrument of this kind are all geometrical, and, consequently, the possible errors are of the same kind. Most optical instruments involve no relationships of speeds or forces, and the calculation of their mechanical systematic errors take very similar forms. For instance, a number of the systematic errors of photography for micrometric purposes resolve themselves into this problem: Given that the principal ray from an object strikes the plate at some given point when the apparatus is in perfect adjustment; assuming the plate to be tilted about its X-, Y-, and Z-axes through assumed angles, at what distance from the original point will the principal ray strike the plate? This is almost identical with the problem already discussed.

THE WIMPERIS ACCELEROMETER

The function of this instrument is to measure accelerations in some one definite direction, and is used to evaluate the accelerations of vehicles such as motor cars, tramcars, locomotives, etc. In addition, the friction forces of the mechanism, including the brakes, can be measured by ascertaining the negative acceleration, or retardation of the vehicles when coasting.

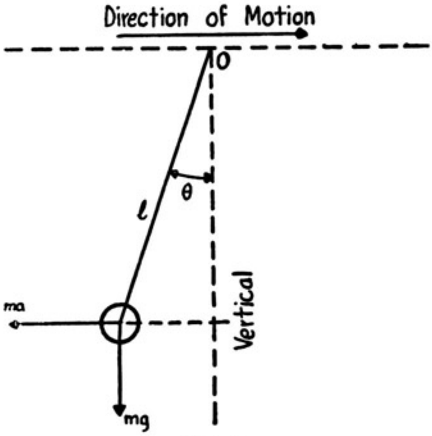

FIG. 8.

If a pendulum be suspended in a vehicle, free to swing in the direction of motion, the angle it makes with the vertical is a measure of the acceleration a. The pendulum, in fact, can be used as an accelerometer. In Fig. 8 the pendulum of mass m and length l is in equilibrium under the influence of two forces, mg acting vertically downwards, and ma in the horizontal direction. The pendulum makes an angle θ with the vertical. The moments of these forces about the point of suspension O are,

$$-mgl \operatorname{Sin} \theta,$$

and

$$mal \operatorname{Cos} \theta.$$

For equilibrium

$$mal \operatorname{Cos} \theta - mgl \operatorname{Sin} \theta = 0;$$

i.e.,

$$\frac{a}{g} = \operatorname{Tan} \theta. \qquad \text{XII}$$

Since g is a constant, θ depends on a only, and is independent of all other factors such as the mass and length of the pendulum.

This instrument has the merit that its accuracy is not dependent on the care taken to determine its mass and dimensions.

To obtain the sensitivity of this instrument for different values of $\frac{a}{g}$ we require to know the change of θ occasioned by a small change of $\frac{a}{g}$ for given values of the latter; or expressed mathematically, the sensitivity, σ, is measured by $\frac{d\theta}{d\frac{a}{g}}$.

From XII;

$$\theta = \mathrm{Tan}^{-1}\frac{a}{g},$$

and

$$\frac{d\theta}{d\frac{a}{g}} = \frac{1}{1+\left(\frac{a}{g}\right)^2}.$$

Thus, when

$$\frac{a}{g} = 0, \qquad \theta = 0;$$

and

$$\sigma = 1.$$

When

$$\frac{a}{g} = 1, \qquad \theta = \mathrm{Tan}^{-1}\,1 = \frac{\pi}{4} \quad (= 45°);$$

and

$$\sigma = \tfrac{1}{2}.$$

When

$$\frac{a}{g} = \infty, \qquad \theta = \frac{\pi}{2} \quad (= 90°);$$

and

$$\sigma = 0.$$

In Fig. 9 the sensitivity curve A is plotted against $\frac{a}{g}$, from which it will be seen that the sensitivity varies continuously, and when a is in the neighborhood of $3g$ the sensitivity has already fallen to one-tenth its initial value. The sensitivity of this accelerometer appears to be useful if a range of a between 0 and about g, or at most $2g$, is under investigation. If greater accelerations are to be measured, the scale gets unduly cramped; while if the required range happens, for example, to be 0 to $0.5g$ or less, a greater sensitivity might be obtained if the scale could be opened out at the beginning.

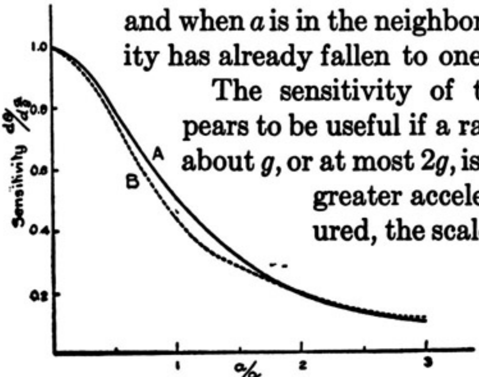

Fig. 9.

A serious objection to the instrument in this form is that it is a single range instrument. Equation XII shows clearly that there is no way of introducing a change of scale, and the dependence of θ on g alone proves a severe limitation.

Suppose the pendulum to be so placed that it lies in a horizontal plane with its pivot vertical, then the force mg will act at right angles to its direction of movement and become inoperative. Such an arrangement could conveniently consist of a disc weighted with mass m at one point, and with its spindle

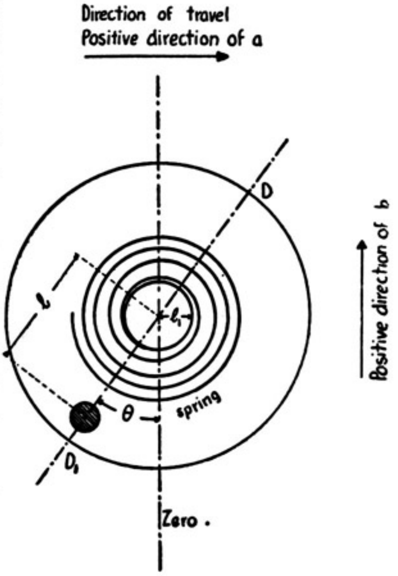

Fig. 10.

vertical as in Fig. 10. Since the pendulum is no longer controlled by gravity, a restoring force is introduced by a spring which, in the absence of disturbing factors, returns the disc to its zero position. Within its working range the force exerted by such a spring is directly proportional to its displacement. Assume the accelerometer is so placed that the diameter DD_1, carrying the weight, is at right angles to the direction of movement; then any acceleration a in that direction will cause DD_1 to take up a position making an angle θ with the zero. The moment of the restoring force about the centre 0 is $-cl_1\theta$; where c is a constant depending on the spring and l_1 is the distance of its attachment from 0. The upsetting moment due to a is

$$mal \operatorname{Cos} \theta, \text{ as before.}$$

For equilibrium,

$$mal \operatorname{Cos} \theta - cl_1\theta = 0;$$

$$\therefore a = \frac{cl_1}{ml} \cdot \theta \operatorname{Sec} \theta.$$

Here a is not only dependent on θ but also on the controllable factors c, m, l. To arrive at the sensitivity of this instrument it will be convenient to obtain the reciprocal; namely,

$$\frac{da}{d\theta} = \frac{cl_1}{ml}\left(\theta \frac{d \operatorname{Sec} \theta}{d\theta} + \operatorname{Sec} \theta \frac{d\theta}{d\theta}\right)$$

$$= \frac{cl_1}{ml}\left(\theta \frac{\sin \theta}{\cos^2 \theta} + \operatorname{Sec} \theta\right)$$

$$= \frac{cl_1}{ml} \cdot \frac{\operatorname{Sin} \theta + \operatorname{Cos} \theta}{\operatorname{Cos}^2 \theta}.$$

Hence the sensitivity is measured by

$$\frac{d\theta}{da} = \frac{ml}{cl_1} \cdot \frac{\operatorname{Cos}^2 \theta}{\operatorname{Sin} \theta + \operatorname{Cos} \theta}. \qquad \textbf{XV}$$

And when $\theta = 0$,

$$\frac{d\theta}{da} = \frac{ml}{cl_1}.$$

And so, if $\frac{ml}{cl_1}$ is arranged to be numerically equal to the value of g expressed in the same units, this instrument will have the same sensitivity for very small values of a as the pendulum accelerometer. Assuming $\frac{ml}{cl_1}$ to have this value, the sensitivity is given by curve B in Fig. 9; this is a trifle less favorable than curve A in the useful range, but the difference is quite negligible. The great gain, however, is that the scale of this curve is under control. If desired, $\frac{ml}{cl_1}$ can be made equal to $0.1g$ (or any other value) in which case the values shown on the base refer to $\frac{a}{0.1g}$, and so on. The advantages of constructing this accelerometer in the form of a disc are twofold: first, its inertia is great compared to that of the superimposed weight; consequently the speed with which θ changes when a varies is very greatly reduced without altering the final value of θ or σ, thus making for steadier indications in the presence of vibration, and this can be still further improved if the disc be made of copper and placed between the poles of a permanent magnet.

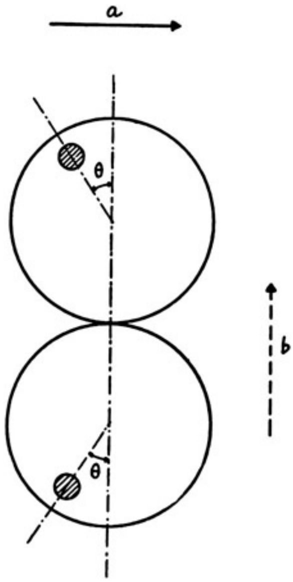

FIG. 11.

This accelerometer suffers from one very serious defect; namely, its readings are not independent of accelerations at right angles to that required. For instance, in Fig. 10 an acceleration in the direction b will evidently affect θ and prevent a correct determination of a.

An extremely neat solution of this difficulty was discovered by Mr. Wimperis who employed it in his patent accelerometer made by Messrs. Elliott Bros.; Fig. 11 illustrates the principle. Two similar discs are geared together, as shown, in such a manner that when one is rotating clockwise, the other is rotating anticlockwise; and the two counter weights, of equal mass and at the same radius, are so disposed that their diameters both make a (numerically) equal angle with the line joining the centres of the discs. One or both of the discs are controlled by a spring as before.

It is easily seen that this arrangement is not affected by the acceleration b, and consequently only records accelerations in one direction. This is an important point, for all vehicles are liable to accelerations at right angles to the direction of their travel, and it is not too much to say that this device made reasonably accurate accelerometers a possibility.

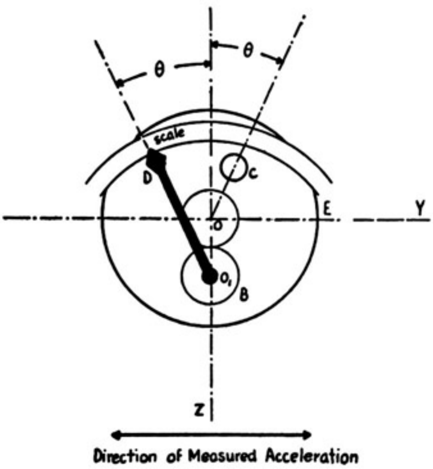

FIG. 12.

The Wimperis accelerometer is arranged as in Fig. 12. The copper disc E carries a spur wheel A which meshes with a similar spur wheel B, the latter carrying the pointer D. C is not a weight but a hole in the disc, and matters are so arranged that the mass of the metal removed at C, multiplied by its distance from O, is equal to the mass of the pointer, multiplied by the distance of its CG from O_1. Hence the moments of the two out of balance masses, about their respective centres of rotation, are equal and opposed as before; but not the total inertias of the two moving parts about their respective centres. The restoring force is supplied by a spiral spring (not shown) and the disc is placed between the poles of a magnet. This instrument is graduated to read positive accelerations in one quadrant and negative accelerations, or retardations, in the other.

Several possible sources of systematic errors exist, and these will be considered, but such errors should always be investigated in relation to the possible accuracy of the experiment to be performed. It is obvious that an instrument of this sort is quite incapable of the type of accuracy demanded from a travelling microscope, nor would such accuracy be of the slightest use for many purposes even if available. Assuming, as obvious, that in the presence of constant accelerations the Wimperis accelerometer will only be affected by those having a component along the Y-axis (Fig. 12), the effect of errors of adjustment will first be considered.

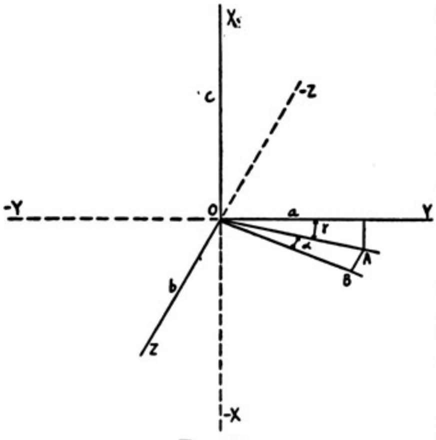

Fig. 13.

In Fig. 13 OY represents the direction in which an acceleration a is to be measured; OZ represents the horizontal direction at right angles to OY, and the vehicle suffers some acceleration b in this direction. OX is the vertical axis, burdened with acceleration C.

The instrument is in correct adjustment when its direction of measurement lies along OY, but suppose this direction has undergone rotations of α and γ about the X- and Z-axes, respectively (rotations of the instrument about its direction of measurement are immaterial), the instrument will then measure the components of the accelerations a, b, and c, in some direction OB; the resultant acceleration measured is,

$$a \text{ Cos } \gamma \text{ Cos } \alpha + b \text{ Cos } \gamma \text{ Sin } \alpha - c \text{ Sin } \gamma \text{ Cos } \alpha.$$

Having regard to the accuracy demanded from such an instrument, and the fact that all angles are small, cosine terms can be neglected and the acceleration measured is roughly:

$$a + b \text{ Sin } \alpha - c \text{ Sin } \gamma.$$

So

$$\epsilon = b \text{ Sin } \alpha - c \text{ Sin } \gamma,$$

and

$$\eta = \frac{b}{a} \text{ Sin } \alpha - \frac{c}{a} \text{ Sin } \gamma.$$

When travelling on a straight course, b can only represent short time oscillations which will be more or less damped out and are not serious. On curves, b can easily equal the maximum values of a obtainable with most vehicles; hence, in terms of a max., η may easily rise to about 0.02 when α has a value of 1°. Apart from short period vibration due to the vehicle's springs c has the numerical value of $-g$, and, since a max. may well be less than $0.2g$, η can rise to 0.1 when γ has a value of 1°.

With a little care α can be kept well under 1° and γ can be controlled to within any significant limits, provided the gradient of the track on which the vehicle is standing be known, by manipulating the adjustable leg provided until the acceleration recorded with the vehicle stationary corresponds to the slope of the track. It must not be forgotten, however, that some vehicles, such as motor cars, may alter their trim fore and aft by more than 2° when accelerating or decelerating.

Another class of systematic errors is that due to imperfect manufacture of the instrument itself. Thus, if the two opposed masses have not equal moments about their respective centres, or are not truly opposed, errors will arise, due to imperfect elimination of accelerations at right angles to that under measurement.

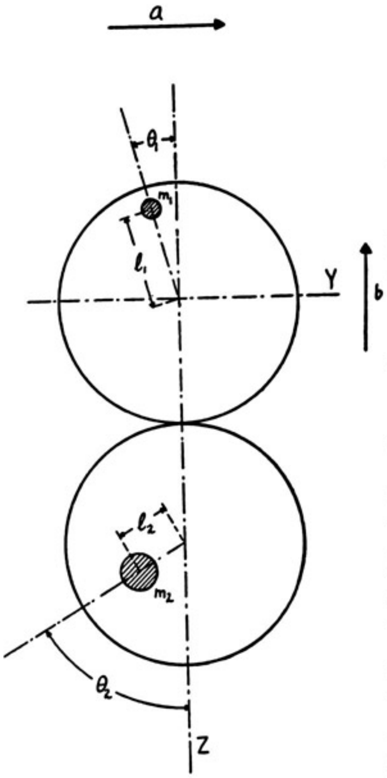

FIG. 14.

In Fig. 14 assume that

$$m_1l_1 \neq m_2l_2,$$

and

$$\theta_1 \neq \theta_2.$$

The total moment of the force tending to deflect the instrument from its zero position due to a becomes:

$$a(m_1l_1 \operatorname{Cos} \theta_1 + m_2l_2 \operatorname{Cos} \theta_2),$$

instead of

$$a(2ml \operatorname{Cos} \theta).$$

Whether this introduces any error is entirely a question as to whether the scale of the pointer is engraved by trial or as a result of some incorrect theory as to the values of m, l, and θ. In any case, a trial of values of a up to $a = g$ is exceedingly easy; since, by tilting the instrument in the direction of measurement by any angle γ, a force is applied to the moving elements which is the equivalent of a, where

$$a = g \operatorname{Sin} \gamma.$$

In the presence of the acceleration b an unavoidable error arises which cannot be eliminated by "faking" the scale.

The deflecting moment due to b is

$$b(m_2l_2 \operatorname{Sin} \theta_2 - m_1l_1 \operatorname{Sin} \theta_1). \qquad \text{XVI}$$

It is seen that two *independent* conditions must obtain in order to make the readings insensitive to b; namely;

$$m_1l_1 = m_2l_2,$$

and

$$\theta_1 = \theta_2.$$

Suppose

$$\theta_2 - \theta_1 = 2^\circ$$

and

$$\frac{m_1l_1}{m_2l_2} = 0.95;$$

then expression XVI becomes

$$am_2l_2 (\operatorname{Sin} \theta_2 - 0.95 \operatorname{Sin} \theta_1).$$

Whilst, if

$$\frac{m_2l_2}{m_1l_1} = 0.95,$$

then the deflecting moment of b is

$$am_2l_2 (0.95 \operatorname{Sin} \theta_2 - \operatorname{Sin} \theta_1).$$

A difference of 2° between θ_1 and θ_2 is an enormous error, and to some extent the resulting error can be corrected by slightly altering either out of balance mass in such a way that the errors due to θ and ml are opposed. This cause of error would be quite negligible but for the fact that only one of the two components carries a restoring spring in the Wimperis accelerometer. The mass of this spring can only be imperfectly centred about its spindle, and is subject to movements, under the influence of accelerations and variations of θ, which are not easy to foresee. Obviously, the spring should be as light and as short as is compatible with its duty. Moreover, it is probable that there exists some angular position of anchorage relatively to m such that the errors due to the spring are a minimum. This is a matter for experiment prior to final manufacture; whilst if a sufficient degree of accuracy is not otherwise obtainable, similar springs could be similarly anchored to each spindle and coiled with opposite hand, in which case their respective errors would cancel out.

Assuming the instrument to be in perfect adjustment, it is clear that the moving parts require time to reach their final position of rest, due to the inertia of their mass and the magnetic damping. Consequently, the reading is only correct in the case of a steady acceleration. In the case of the Wimperis accelerometer this also applies to angular changes of acceleration, for, although the out of balance masses are balanced as regards their first moment (though not necessarily as regards their moment of inertia), the total moment of inertia of the disc far exceeds that of the pointer. Consequently, if placed in a train travelling at a constant speed, the instrument will register a false acceleration when the train runs on to or off a curve. The sign of this false acceleration depends on the hand of the curve. It is interesting to note that had the instrument been designed as in Fig. 11 the correctness of the readings would be independent of angular changes of acceleration, although not independent of the constancy of the linear acceleration to be measured. In practice, this is probably not an important point, provided it is not desired to evaluate the acceleration of a vehicle on a curve.

Suppose again that the instrument is in correct adjustment, to what extent is its accuracy dependent on a knowledge of the gradient of the track or course? The answer to this entirely depends on the information that is required. From previous calculations it is clear that, if the vehicle is on a gradient whose angle is γ, the instrument will register the correct acceleration a, $\pm g$ Sin γ; + if the vehicle is rising, and − if falling. Hence the instrument will only register the correct acceleration on the level. Suppose that on the level the vehicle is capable of an acceleration a, when developing a given power at a given speed, then on a gradient whose angle is γ its acceleration, under the same conditions of speed and power, becomes

$$a \mp g \text{ Sin } \gamma; \ - \text{ if rising and } + \text{ when falling.}$$

Hence the instrumental error due to gradient exactly counters the effect of the gradient on the vehicle, and the instrument correctly states the acceleration that the former would be capable of on the level, other conditions remaining the same. Since the object of testing the acceleration of a vehicle is usually to deduce some information about the vehicle itself, this apparent defect of the instrument is in reality one of its greatest merits.

ASSAY BALANCE

The third instrument we shall consider is an assay balance, as used for accurate work in chemical and physical laboratories.

A balance consists essentially of four parts: the frame; the beam, carrying the fulcrum and two knife edges, one at each extremity; and two pans depending from the knife edges.

Such a balance is used for the comparison of masses by equating the forces acting on these masses due to the gravitational field. First, the mass to be weighed is roughly balanced against one or more standard masses until a close approach to a null reading is obtained. Secondly, to obtain a more precise reading, the null method is abandoned; the deflection of the beam from its zero position is noted, and the difference between the unknown and known masses deduced therefrom. This deflection is commonly evaluated before the balance has come to rest by noting the pointer readings at each end of a few swings and taking the mean. A balance working in air does in fact compare negative buoyancies rather than masses, for if equal masses do not occupy equal spaces they will not equate, the apparent difference being

$$(M_1-m_1)-(M_2-m_2)=m_2-m_1=\text{inequality},$$

where m_1 and m_2 stand for the masses of air displaced by the equal masses M_1 and M_2, respectively. This inequality is dependent on the density, or mass per unit volume, of the air, which is not a constant. Since g is not a constant at different places, or at different heights in the same place, the weight gM of a mass M is not constant unless it is kept in one place and height. On the other hand, it is customary to refer to the known masses by which the process of weighing is accomplished as "weights", and these "weights" are used as constants independent of their situation. This is justifiable so long as it is understood that it is the mass of these weights that is being referred to; for (neglecting the presence of air) if a known "weight" W_k balances an ununknown weight Mg it follows that, since $W_k=M_kg$, what is observed is, $M_kg=Mg$; and, since M_k and M are substantially in the same situation, g will be the same for both, therefore $M_k=M$.

When known masses are referred to they will be described as "weights" in inverted commas, and it will be further assumed for simplicity that all weighing takes place in vacuo.

In Fig. 15 *AFB* represents a beam suspended from its fulcrum *F*, whilst two pans (not shown) are depending from the knife edges *A* and *B*. The height of the fulcrum above the knife edges is *h*. It will be assumed to begin with that no errors of any sort are present. Since the pans depend freely from *A* and *B*, the first moments of their masses (including their contents) about F in a vertical direction are exactly the same as if those same masses were concentrated at A and B, respectively. Let the mass of the beam, together with the empty pans, be the constant quantity M_c with its CG at G_c, somewhere on the perpendicular through F to the line AB. Assuming that both pans are empty, G_c will lie on the vertical through F, and $\theta=0$. If a very small "weight" of mass m, placed in pan B, causes the beam to rest at some angle θ to the zero position, then the sensitivity of the balance unloaded is $\frac{d\theta}{dm}$; and in general, if each pan is loaded with a known "weight" M_1, and one of the pans receives a small additional mass m, the sensitivity of the balance when weighing M_1 is $\frac{d\theta_1}{dm}$. Now G_M, the intersection of AB and FG_c (produced if necessary), is the CG of the two masses M_1; hence, if each pan is loaded with M_1, and one pan carries the very small mass m in addition, the condition for stability is,

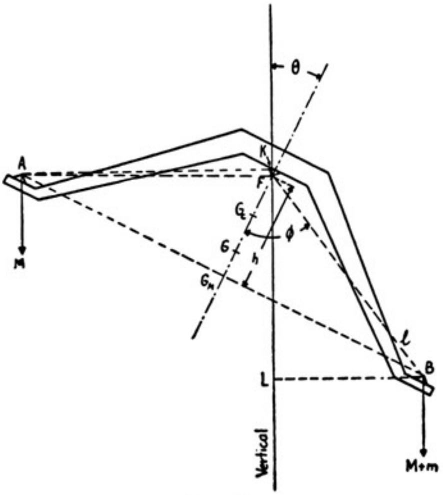

FIG. 15.

$$(M_cFG_c+2M_1FG_M)\ \text{Sin}\ \theta_1=ml\ \text{Sin}\ (\phi-\theta_1);$$

or,

$$m=\frac{\text{Sin}\ \theta_1}{\text{Sin}\ (\phi-\theta_1)}\cdot\frac{M_cFG_c+2M_1FG_M}{l}\cdot$$

In practice ϕ is very nearly $\frac{\pi}{2}$, and θ_1 is kept small; hence, to a first approximation,

$$m=\theta_1\left(\frac{M_c\,.\,FG_c+2M_1\cdot FG_M}{l}\right).$$

$\therefore$ Sensitivity,

$$\frac{d\theta_1}{dm}=\frac{l}{M_c\cdot FG_c+2M_1FG_M}. \qquad \text{XVII}$$

It will be seen that, other things being equal, the sensitivity varies directly as the length of the beam. Other things never are equal, for the weight of a beam increases with its length; and the first term in the denominator indicates that the sensitivity decreases as the moment of the beam (and pans) about F increases, assuming, what is always the case, that G_c lies below F. In spite of this, the most sensitive balances are usually of the long beam type, although there are exceptions. The second term under l shows that the sensitivity decreases as the load, $2M_1$, on the beam increases, assuming, what is not always the case, that G_M lies below F. As a matter of fact, in balances such as this, an attempt is usually made to keep the sensitivity independent of the load, $2M_1$. This occurs when the expression $2M_1\,FG_M=0$ in Equation XVII. That is, when the two knife edges and the fulcrum are in line; this is often done, but, in some cases, the knife edges are actually set a trifle above the fulcrum in order to allow for the inevitable bending of the beam when under load. In this case, $2M_1FG_M$ is negative in XVII for small loads, and the sensitivity increases with M at first. If the knife edges are set too high the sensitivity becomes infinite and the balance is in neutral equilibrium, then any further load will make the sensitivity negative and the balance unstable. G. A. Owen[1] gives a curve of sensitivity for a delicate balance in which the sensitivity first increased 37% as the beam was loaded up to 100 grains, and then fell to 17% below its original value with a load of 700 grains.

[1] *A Treatise on Weighing Machines* by G. A. Owen.

These changes of sensitivity due to the flexure of the beam produce no errors provided that null readings are being obtained; but, as explained above, the last significant figure is obtained by noting the departure of the beam from its null position, and any change of sensitivity will cause an error unless the balance is calibrated for sensitivity against different values of M. It will be assumed that this is done, and the errors of a balance will be investigated in which the knife edges and fulcrum are intended to be in line; the flexure of the beam will be ignored, or rather is assumed to be discounted by means of a calibration curve. Again, as an average example, we shall assume a balance with a maximum capacity of 1,500 grains, capable of measuring to 0.0015 grain, or 1 part in a million on full load. The beam length, $2l$, will be taken as 12 inches and its weight (with pans) 500 grains; this is a fairly long beam for the capacity; the average sensitivity will be taken as 1° deflection for 0.006 grain; then the distance, h, between the instrument's unladen CG, namely G_c, and F works out at about 0.0036 inch.

The first thing to be clear about is what constitutes an error in the performance of such an instrument. A sensitivity only moderately different from that intended will not be taken to cause any error, as explained above. A slight departure from the zero position of the pointer when the pans are empty can be either corrected or allowed for and causes no error.

What is required is that any mass M_1 placed in either pan shall be correctly evaluated by summing the "weights" in the other pan + the calibrated value of the pointer's deflection from its own zero. The "weights" will be taken to be accurate, or at least the actual mass of each "weight" would be known to any accuracy required.

Fig. 16 shows the balance in adjustment, whilst Fig. 17 represents the same instrument with a number of defects grossly exaggerated. F lies above AB; l_a, l_b, and hence the angles α and β, are supposed unequal. The line FG_c, the vertical when the balance is unloaded, makes an angle ϕ with the line through F normal to AB. The pointer is set to zero with the pans empty and an error in the measurement of a mass M_1 consists in M_1-M_2; where M_2 is the "weight" necessary to balance M_1, the pointer remaining at zero. Taking moments about F, and remembering that the forces always act vertically downwards,

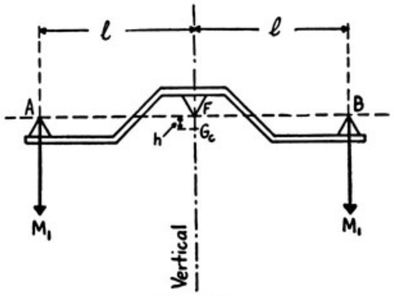

FIG. 16.

$$M_1 l_a \operatorname{Cos}(\alpha-\phi)=M_2 l_b \operatorname{Cos}(\beta+\phi). \qquad \text{XVIII}$$

Now α and β are known to be exceedingly small, since, from the numerical data given above, if α and β reached a value of about ±1½ minutes, the balance would either become unstable or have its sensitivity halved; hence to a good approximation,

$$M_1 l_a = M_2 l_b,$$

or

$$M_1 = M_2 \frac{l_b}{l_a}.$$

$$\therefore \quad \epsilon = M_2\left(\frac{l_b}{l_a}-1\right), \qquad \text{XIX}$$

and

$$\eta = \frac{l_b}{l_a}-1. \qquad \text{XX}$$

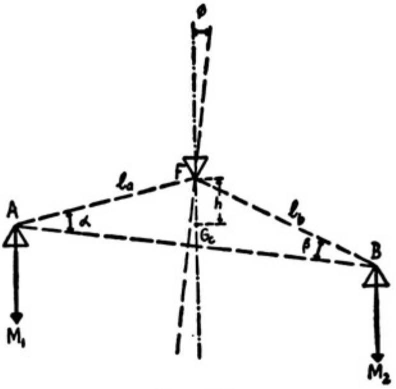

FIG. 17.

Hence, we see that the angles α and β are held to close limits for reasons of sensitivity; a variation of half a thousandth of an inch of F above or below AB alters the sensitivity by roughly 14%. Consequently, the errors of the instrument are almost independent of α and β and are quite independent of ϕ acting alone, though from XVIII the importance of α and β rises with the value of ϕ.

Practically the accuracy of the instrument depends on the equality of l_a and l_b, ϵ being directly proportional to the difference between their ratio and unity. Since a good assay balance in adjustment is accurate to one part in a hundred thousand, and reads to one part in a million on full load, it follows that the permissible difference in length is 6×10^{-5} inches—a remarkable degree of accuracy. This is attained by a laborious process of trial and error. As a secondary consideration ϕ must be small, and this is comparatively easily achieved by slightly rubbing down the heavier arm of the beam, or pan, by trial and error.

Another consideration is the parallelism of the two knife edges and fulcrum; if these are not parallel, the effective lengths l_a and l_b depend on the particular spot on the edges at which the pressure is centred. The possible variation in either arm depends on $k \text{ Sin } \gamma$, where k is the length of the knife edge and γ the angle between that edge and the fulcrum; this is adjusted by trial and error.[1] The most critical requisite of a sensitive balance is clearly equality of lengths or arms, and, as a necessary corollary, parallelism of the three edges. Everything else is easy compared to this. For this reason, all the edges are placed on the beam and never on the other components. Again both knife edges are adjustable for length and horizontal angle, and are independently adjusted parallel to the fulcrum. Sometimes an additional adjustment for height is provided. In addition, the pans hang from the knife edges by a device which enables the former to swing in a direction at right angles to the length of the beam, in order that the mass of the pans and their loads shall be supported evenly along the length of the knife edges. Finally, the design of the beam itself has received great attention from the point of view of rigidity, stability of dimensions and lightness. Other problems connected with the design of a balance are concerned more directly with such matters as convenience of manipulation, durability, etc.

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[1] For information on this subject see E. A. Griffiths, *Engineering Instruments and Meters*, Chapter VI.

FALSE ELEMENTS AND SYSTEMATIC ERRORS

A number of systematic errors have been considered, all resulting from false elements.

An element can be false for two reasons:

a. The designed relation does not lead to the required result. This might be described as a "false design".
b. The design is not rigidly adhered to.

Systematic errors are distinguished from other types by their obedience to regular, and often ascertainable, laws as distinct from chance. In other words, a systematic error is a function of some one or two variables as distinct from the hundred and one hidden causes which are collectively referred to as "chance." An instrument burdened only with systematic errors has the appearance of doing something quite definite; it "knows its own mind", in the sense that it is possible to understand an observation taken with its help in terms of ascertainable data. The question as to whether such an instrument is accurate or inaccurate entirely depends on what information is required, since it is at least behaving with precision.

Thus, the Wimperis accelerometer, as used in a vehicle on a straight hill accelerating uniformly, gives accurate information as to the acceleration which the vehicle would have undergone in similar conditions on the flat, but inaccurate information as to the actual acceleration of the vehicle. It is sometimes argued that this is not an instrumental error but rather an example of an instrument being put to an incorrect use; this is largely a question of words and depends on the uses for which the instrument is in fact the best available.

Occasions in which this accelerometer shows the results of "false design" are when the linear acceleration is not uniform, and in the presence of varying angular acceleration. The latter could have been eliminated by arranging the instrument such that the moment of the two opposed masses (as distinct from the small out of balance masses) were equal; this would not have been difficult in practice and it is presumed that it was not thought worth doing. The correction for error due to changes of linear acceleration cannot be completely effected, depending as it does on the lag introduced by damping, and a compromise between this error and steadiness of reading is inevitable. These are instances in which the designed relation is dependent on some unwanted variables; track gradient, $\dot{\alpha}$ and $\ddot{\theta}$. The observation in fact is not sufficiently isolated.

On the other hand, an element may be isolated in the sense that its relation is reasonably independent from unwanted variables but is not designed to be quite the required relation. Sometimes this is done knowingly and it must be presumed in these cases that the resulting error of observation has been calculated and found to be negligible. Thus; approximate straight line motions in connection with indicators and other instruments, and the following: Suppose in an instrument the linear displacement of one component is required to be proportional to the sine of the angular displacement of a crank shaft, the crank may actuate the first component by means of an infinitely long connecting rod (or its kinematical equivalent); alternatively the connecting link may be a short rod, in which case a definite and easily calculable departure from the sine law will result.

The second cause of systematic errors is that the design is departed from; including under this head, manufacture, adjustment, and conditions of use. For instance, centring errors in graduated circles or drums, as in the case of the comparator considered above; balances with unequal arms; a spring balance with the spring of greater or less stiffness than designed; the two masses in the Wimperis accelerometer not being accurately opposed. The last example not only causes the required acceleration to be incorrectly measured, but to a far greater extent reduces the isolation of the observation by causing the reading to be dependent on accelerations at right angles to the measuring direction. These are all matters for the manufacturer, but very similar errors result from maladjustment. Thus, the adjustments of the comparator have been considered, and those connected with the levelling and direction of the accelerometer's line of measurement.

Since it is impossible to design or make an instrument entirely insensitive to all unwanted variables, its systematic errors are partly dependent on the conditions with which it is surrounded; the most common of these variables is undoubtedly temperature, to which the sizes of all components are related in a greater or smaller degree.

In all these instances of systematic errors the actual elements are not identical with the required elements for one reason or another; but in all cases the two links forming an element had the required degrees of freedom, neither more nor less. The characteristic marks of this type of error are that they are caused by a very limited number of variables, whilst the degrees of freedom of the links forming the events are precisely those required.

It has already been seen that the systematic error present in a measurement may be made less than those composing its observations by adopting a satisfactory method of measurement. Thus, a differential measurement by dispensing with the instrument's zero also eliminates any zero error present. This might be described as comparing two quantities rather than making an absolute measurement of either. In the case of a length the two quantities l_1 and l_2 are compared, i.e.,

$$L = l_2 - l_1,$$

the absolute value of either quantity is of no importance. When a mass is weighed in a spring balance by a differential reading, the result,

$$M = m_2 - m_1,$$

can be regarded as a comparison of two masses. In the case of measurement by substitution, the comparison is carried one stage further and with still better results.

This discussion suggests a golden rule: *Never measure more than is absolutely necessary*. An excellent example was given above in connection with the design of a travelling microscope intended to measure the difference between pairs of photographs; had the lengths to be compared been independently measured, the systematic errors would have been far greater than was actually the case. Another instance is the chemical balance with unequal arms but otherwise well made. Unequal arms result in all determinations of mass being incorrect by the same ratio, if the method of direct weighing is employed, and *provided the known and unknown masses are always placed on the same pans, respectively*. I am not a chemist but I am assured that chemists rarely require to know the mass of anything, though they sometimes think they do.[1] What is required in chemistry is an accurate knowledge of the ratios of the quantities involved. Thus, if a chemist wishes to combine 1 gm. of X with 3 gms. of Y, it does not matter if he accidentally weighs out 1.001 gms. of X provided that he makes the mistake of combining it with 3.003 gms. of Y; and this is exactly what an unequal armed balance will cause him to do. If, however, the product, namely 4 gms. of Z, is to be dissolved in 200 cc. of water, the trouble begins. Here again, only a ratio is involved, but the water may be measured by bulk and not weighed on the balance, hence our chemist will fail to provide the necessary 200.2 cc. In this case, therefore, in spite of only requiring certain ratios, chemists have to be capable of evaluating actual quantities because the same instrument is not used throughout. This is a reason for providing accurate chemical balances for general work. Nevertheless, this illustrates the point; namely, that every additional piece of information required is another occasion for error, and that if, say, a ratio of masses is required, the method should not be such as to involve an accurate knowledge of each mass. Another example of this is seen in the case of a pair of stereo-cameras used for micrometric purposes. The use of such a pair is, by taking simultaneous pictures of an object from slightly different positions, to deduce the distance of that object from the cameras by means of the displacement of the object in one picture as compared with its position in the other. A pair of stereo-cameras is in fact a sort of range finder, using the same basic principles, and has the advantage that a number of objects can be photographed quickly and their distance worked out afterwards. This arrangement is sometimes used for rapid survey. The scale of the picture depends on the distance between the photographic plate and a given point in photographic lens; this is called the "principal distance". An error of 0.1% in the principal distance of one camera will often cause a hopelessly large error in the subsequent work, unless the other camera suffers from the same defect, in which case the resultant error becomes 0.1% and no more. This is an instance in which two absolute measurements of principal distance are required for accuracy, but the accuracy is far more sensitive to the ratio of these measurements than to their absolute values. Advantage is sometimes taken of this, in that it is easier to equalize the principal distances of two cameras than to measure them independently.

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[1] In fairness to the profession I should state that my informant was a chemist himself.

Systematic errors are the easiest of all errors to eliminate, when known, from their regularity and obedience to ascertainable laws. But they present one very great danger, namely, these very qualities enable them to remain hidden and so unsuspected; the precision of a set of observations gives no clue; and the only means of detection usually involves realizing their possibility and devising some method of measurement which will cancel the expected errors, or some other measurement (not necessarily of the quantity under investigation) which will show them up. The possibility of doing this is very largely dependent on the design. The various systematic errors are not all on the same level as regards their elimination, in that they have very different degrees of permanence. Those which are dependent on a difference between the required and designed elements are constants of the instrument and can be known once for all, and, with suitable semipermanent adjustments, probably eliminated. This also applied to those systematic errors resulting from errors of manufacture, although, in either case, the error may also be a function of some other variable, as in the case of the Wimperis accelerometer with incorrectly opposed masses.

In the case of systematic errors resulting from the observer's adjustments, the errors are only constants of that particular adjustment, and they often can only be evaluated by breaking the adjustment down. Hence, it is of great importance that a frequently performed adjustment shall be capable of being set sufficiently accurately to have no significant errors. It is often better, where possible, to build the instrument without an adjustment than to provide an inadequate one, even if the adjustment results in a rather smaller error; for it is frequently impossible to evaluate this error without an unjustifiable expenditure of time. One means of controlling errors of adjustment is to repeat the observation a number of times, breaking down and resetting the adjustment each time, and subjecting the resulting readings to the methods applicable to unbiased short period errors.

Thus, the means of controlling systematic errors are:

1. Better design
2. Better manufacture
3. Observers' adjustments with the means of checking
4. Adjustment of each observation or measurement by calculation
5. Turning the errors into short period unbiased errors and dealing with them as such

The last two methods are a confession of failure on the part of the designer and should only be tolerated as a last expedient. The third method often cannot be entirely avoided but should be reduced to a minimum. It must be remembered that, to the observer, an instrument is only a necessary evil, the means to an end, which has nothing to do with the instrument itself; and he may have neither the time nor the detailed knowledge necessary to coax an inadequate instrument into good behavior. Equally it is the object of a good design not only to be adequate if perfectly executed but to give good results when manufactured to reasonable limits of accuracy.

SLIDING OR ROLLING SURFACES

There is one type of false element which may lead to errors not possessing all the characteristics of systematic errors; namely, those comprising two links sliding or rolling on each other in which one or both surfaces do not conform to the design.

Thus, a light carriage, or slide, may consist of a platform with three legs, two of which slide in a *V* groove, whilst the third slides on a plane. If the "plane" be in fact significantly rough or out of truth, the slide will rise and fall instead of pursuing an even course; the departures of the slide from plane motion may be described as systematic errors in that they are a function of the mechanism, and could nominally be predicted from a prior knowledge of the sliding surfaces. Moreover, subject to wear, these errors are invariable in the sense that a given position of the slide is always associated with some definite error.

In practice, it would usually be quite out of the question to explore these errors with any hope of knowing the approximate error associated with any definite position; the irregularities of the surfaces are generally too numerous and unpredictable. Consequently, for all practical purposes, such errors are unknown, both before the setting of the slide and probably afterwards. Fortunately, however, the slide is behaving as though disturbed from some one fairly definite plane along which it may be regarded as tending to move, and this plane is defined by the general trend of the sliding surface. Thus, the errors are not erratic, for no element is underconstrained and the errors tend to cluster round some mean value; or in other words, some definite element may be regarded as indicated. For these reasons, they can often be regarded as short period errors; and, by repeating an observation several times, taking care not to employ exactly the same parts of the slide on the various occasions, the worst of the resulting inaccuracies may well cancel out.

A ball rolling in an inaccurate race will evidently behave in a similar manner to the slide just considered, but in a ball bearing the irregularities of the paths of the inner and outer races with respect to each other are greatly reduced by being an average result of a number of rolling balls.

For the same sort of reason, one surface sliding over another may show smaller errors of motion than if one of the surfaces be reduced nearly to a point. This matter is considered again in Part II in connection with kinematic design.

It is not always, however, that errors of surface are so irregular as to defy systematic rectification either by means of an additional adjustment or by subsequent computation. Reference has already been made to the periodic errors of accurate screws as used for micrometric purposes; these errors, which tend to be a more or less exact copy of those of the master screw in the lathe on which they were cut, often repeat themselves at various regular pitches. The most serious of these has usually a pitch equal to that of the screw and is often due to the eccentric mounting of the master, or the work. Since the nut of such a screw would normally cover a length of over 10 times the screw's pitch, a periodic error recurring at any shorter interval is evened out and so rendered harmless. Smaller periodic errors of longer pitch are usually present, and, for the most accurate work, these are significant; they are often corrected by causing the nut, which normally traverses the screw without rotating, to turn very slightly in one direction or another as it moves along the rotating screw. A plate is fixed to the bed of the instrument parallel to the screw; and the nut, imparting motion to a slide, has an arm which rests on the edge of the plate and moves along the latter as the nut travels. The edge of the plate is not quite straight, and the nut is consequently caused to rotate or oscillate slightly as it moves along; this advances or retards the nut on the screw, and is arranged to counterbalance the errors of the screw itself. This device is used on some of the finest travelling microscope measuring instruments, and also on lathes used for cutting the most accurate screws.

Irregularities in very accurate polished surfaces take the form of very shallow elevations or depressions of relatively great area, comparable to the surface of a slightly waved sheet of smooth paper. This sort of surface error is not difficult to detect by observing the interference fringes between the surface in question and that of a plane plate of optical glass laid over it. The high spots can be rubbed down with rouge or similar material until a surface of many square inches is rendered plane to within half a wave length of light (roughly 0.5 μ); whilst a very skillful optical glass worker can render such a surface plane to something even less than this.

Nevertheless, in spite of a few exceptional cases, it is generally the case that irregularities in sliding surfaces, where they do not even out, produce errors which must be regarded more as short period than systematic errors. For, although a constant of the element, they are too irregular to be explored; and errors due to any two relative positions of the sliding links bear no discoverable relation to each other.

CHAPTER III

PROBABILITY AS APPLIED TO ERRORS

GENERAL

When an instrument is designed and made it will be subject to the possibility of error, and it has already been seen that a correct method of measurement, and possibly calculation, will greatly minimize the size of the final error.

Two kinds of error are particularly susceptible to control in this way: systematic errors, which have already been considered, and unbiased errors. The actual errors that occur, apart from systematic errors, are erratic and short period errors, and unfortunately these do not always correspond with the mathematicians' unbiased errors. Before discussing these actual errors it will be well to consider the mathematical theory of unbiased errors. No proofs will be given; these will be found in any of the standard textbooks on the subject; but the characteristics of unbiased errors and the data assumed will be briefly reviewed in this chapter in order that the theory may be used legitimately in the following chapters.

The sense in which an error can be unbiased requires some consideration, for obviously at any one instant any error present must be biased, in that it can occur once, and once only, with a definite sign, and a definite magnitude, ϵ.

Suppose that on n successive and similar measurements of a given quantity, all performed with the same instrument under seemingly similar conditions, the errors of the readings are found to be

$$\epsilon_1, \epsilon_2, \cdots, \epsilon_n.$$

Then if the ratio

$$\frac{\epsilon_1,\ \epsilon_2,\ \cdots,\ \epsilon_n}{n},$$

tends to zero as n increases indefinitely, the measurements are said to be subject to unbiased errors.

ASSUMPTIONS

The mathematical theory depends on certain assumptions as to the character and origin of these unbiased errors.[1]

1. The errors are supposed to be graduated errors, as distinct from errors of integers. Thus, an estimate of the number of persons in a lecture hall is subject to an error of integers, in that the error must be expressible in whole numbers, the least count being one person. An estimate of distance can, ideally speaking, proceed by any graduations, however minute, and is, therefore, said to be subject to graduated errors.
2. An unbiased error of observation is the algebraic combination of an indefinitely large number of partial errors, each partial error having an independent cause.

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[1] These assumptions are not stated with complete logical exactitude. In the first place, the remaining conditions appear to have no practical bearing on the design of instruments; and secondly, I can find no writer who gives an adequate discussion of the whole class of things which obeys the normal law of probability; each writer selects a particular member of this class and proves the law for that case. Thus Poincaré "Calcul des Probabilities", ch. XI, p. 201: "L'erreur commise avec un instrument est la résultante d'un très grand nombre de petites erreurs indépendantes les unes des autres, et telles que chacune d'elles n'entre que pour une faible part dans le résultant; l'erreur résultante suivra la loi de Gauss". Later on, p. 206, Poincaré adds, referring to the same errors "..., qui serant sensiblement du même ordre de grandeur, ...". Airy assumes that the probability curve for each partial error is a straight line parallel to the base, i.e., all sizes are equally likely within the possible limits. Hagen, as quoted by Merriman and by Brent, assumes that all partial errors are numerically equal.

Gauss is quoted by Merriman, and by Poincaré, as basing the law of error on the assumption that the most probable value of a quantity, which is observed directly several times with equal care, is the arithmetical mean of the measurements. This seems to be the most general statement, but completely evades any discussion of the nature of partial errors.

Keynes, in his *Treatise on Probability*, barely touches the question.

3. Partial errors may be of different magnitudes, but the largest partial error must be indefinitely small in comparison to a likely unbiased error of observation.
4. Each cause shall independently be equally liable to produce a partial error of either sign.
5. The probability curve for the possible magnitudes of a partial error can assume any form provided it is symmetrical about its zero axis, and is contained within the limits set by (3). The probability curve for any error is that curve representing the relative chances of the error taking any of its possible magnitudes.

Assumptions (2) and (3) taken together really amount to this: That any one partial error must be so small, in comparison to the average run of its unbiased error of observation, that if it be removed the change in the final error is negligible. Common sense indicates that a statistical law, like the laws of probability, cannot be expected to operate unless the idiosyncrasies of individuals are swamped by the mass of the average. Speaking loosely, this implies two things: The number of individuals must be very large, and no one must tower above the others to such an extent as to impress its own peculiarities on the average to a noticeable extent.

Assumptions (4) and (5) imply that a partial error can be what it pleases provided it is small, independent, and unbiased.

The theory of probability was worked out in some detail by the French mathematician Laplace, at the beginning of the nineteenth century and applied more particularly to errors of observation by Airy, the English Astronomer Royal, about 50 years later.

Since the laws of probability are essentially statistical, they cannot be expected to apply to a single event considered alone, but indicate the average behavior of a group of events, which *average* is a definite property of the group.

When a group of unbiased errors of similar observations is indefinitely large, the character of this group can be expressed in one invariable mathematical form, with suitable constants; thus, all such groups have the same character apart from scale.

If indefinitely large groups of similar observations were available, the character of any such group could be definitely deduced; but this never is the case, for, in practice, the number of similar observations forming a group is almost invariably small, and it is assumed that this small group is a representative sample of what would occur if the observation were indefinitely repeated.

DEDUCTIONS

On this assumption a number of deductions follow from the application of the theory of probability to unbiased errors, as defined above; and a few of these, the most important for the purposes of this book, will now be enumerated.

1. If an observation be repeated a number of times, under equally favorable conditions, then the most probable value for that observation is the arithmetic mean (a.m.) of the various readings. The same applies to the most probable value of a quantity measured a number of times under similar conditions.

   (Definition)

   If R_M be the mean reading of n similar observations,

   $$R_1, R_2, \cdots, R_n,$$

   then R_1-R_M, R_2-R_M, $\cdots$, R_n-R_M, are called the residuals of the readings, and are, by (1), the most probable errors of their respective observations; they will be treated as real errors and designated ϵ_1, ϵ_2, $\cdots$, ϵ_n, and it can be shown that they obey the same Law of Probability as the real errors associated with the readings when n is large. The distinction between residuals and actual unbiased errors is often forgotten with unfortunate results. Clearly $\epsilon_1+\epsilon_2+\cdots+\epsilon_n=0$, for any group of observations, or measurements.

2. The probability that the magnitude of an unbiased error of observation will fall between ϵ and $\epsilon+\delta\epsilon$ is given by the expression:

$$\frac{1}{C\sqrt{\pi}}\cdot e^{-\frac{\epsilon^2}{C^2}}\delta\epsilon; \qquad \text{XXI}$$

where e is the base of Naperian logarithms

$$(e=2.71828\ \cdots);$$

C is called the modulus of the observation, and is a measure of its precision. All measurements having the same precision will have the same numerical value for C.

Values of $e^{-\frac{\epsilon^2}{C^2}}$ in terms of $\frac{\epsilon}{C}$ have been calculated, and Table I gives a few values abstracted from Airy's *Theory of Errors of Observations.*

TABLE I

VALUES OF $e^{-\frac{\epsilon^2}{C^2}}$

$\frac{\epsilon}{C}$	$e^{-\frac{\epsilon^2}{C^2}}$	$\frac{\epsilon}{C}$	$e^{-\frac{\epsilon^2}{C^2}}$
0.0	1.0000	2.6	0.001159
0.2	0.9608	2.8	0.0003937
0.4	0.8521	3.0	0.0001234
0.6	0.6977	3.2	0.00003571
0.8	0.5273	3.4	0.000009540
1.0	0.3679	3.6	0.000002353
1.2	0.2369	3.8	0.0000005355
1.4	0.1409	4.0	0.0000001125
1.6	0.07731	4.2	0.00000002183
1.8	0.03916	4.4	0.000000003909
2.0	0.01832	4.6	0.0000000006461
2.2	0.007907	4.8	0.00000000009860
2.4	0.003151	5.0	0.00000000001389

The curve shown in Fig. 18 is obtained by plotting $e^{-\frac{\epsilon^2}{C^2}}$ on a base of $\frac{\epsilon}{C}$ and shows the comparative frequency of errors of various sizes, the latter being expressed as a ratio of the modulus. Since positive and negative errors are equally likely, the curve is symmetrical about the Y-axis, and errors of both sign are included in the figure. A change in the modulus C alters the scale of the abscissa and corresponds to measurements of greater or less precision; hence, before any use can be made of the curve in a practical case, C, or some function of C, must be evaluated.

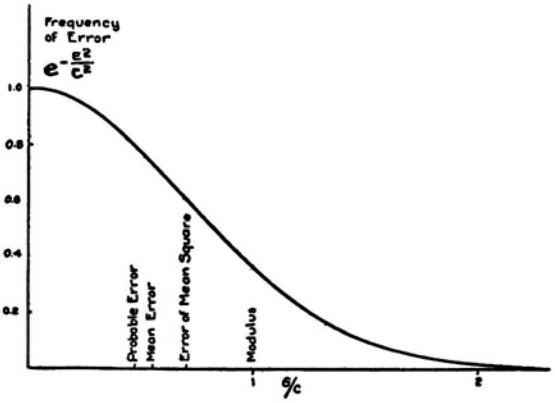

FIG. 18.

Since the form of the curve is invariable its various geometrical properties will always bear a constant relationship to each other. The position of C is one of these properties and corresponds to that point on the curve where $\frac{\epsilon}{C}=1$. The position of the probable error (p.e.) is another such property and is defined as that value of $\frac{\epsilon}{C}$ for which as many errors numerically exceed the p.e. as fall short of it; the value of

$\frac{\epsilon}{C}$ corresponding to the position of p.e. is found to be 0.4769. The mean error (m.e.) is defined as the average value of the errors (or residuals) taken without respect to sign. The value of the mean error is 0.5642.

Similarly the value of the square root of the mean of the squares of errors, called error of mean square (e.m.s.), is 0.7071. It will be noticed that the m.e. and e.m.s. correspond to the abscissa values of the first and second moments of the area under the curve respectively.

These geometrical relationships[1] are repeated for convenience:

$$\text{p.e.} = 0.4769\ C.$$
$$\text{m.e.} = 0.5642\ C.$$
$$\text{e.m.s.} = 0.7071\ C.$$

Since the modulus C, and the other quantities given above, are all related as shown, a knowledge of any one of them enables the others to be evaluated.

The term "probable error" is really a misnomer, for an error of this magnitude is in no sense peculiarly probable; what is probable is that as many errors will be numerically greater as smaller than the p.e.

In practice, a small group of observations (or measurements) cannot have residuals whose frequency is quite typical of the group of such observations repeated indefinitely. Thus, a residual of such a magnitude that it is only expected to occur once in a thousand times cannot have its frequency correctly represented in a group of a hundred observations. Nor can a limited group or sample be expected to be quite as typical of its "population" as is theoretically possible. Thus, if an unbiased coin be tossed 101 times, the best it can do, to indicate the result of an unlimited number of tossings, is to give heads on $\frac{50}{101}$ occasions, and tails on the remaining $\frac{51}{101}$ tosses; or vice versa. Actually the approach to half and half will probably be less close than this. Hence, if for a group of observations, the modulus C be calculated from the m.e., and again from the e.m.s., and finally from the p.e., three somewhat different values of C will result. It can be shown that the e.m.s. gives the most probable result, that of the m.e. being only slightly worse. Since the latter involves far less trouble it is usually, though not always, used. When the object of obtaining C is simply as a measure of the precision, or "scatter", of a group any of the other geometric properties of the probability curve will do equally well, and it is usual to quote one of these.

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[1] Continental and American writers usually refer to the m.e. as the "probable error", and the e.m.s. as the "mean error".

3. Another deduction from the theory of probability is that if Z be the sum of a number of fallible results $P, Q, R, \cdots$, then,

$$(\text{m.e. of } Z)^2 = (\text{m.e. of } P)^2 + (\text{m.e. of } Q)^2 + (\text{m.e. of } R)^2 \cdots.$$

It follows that, if $P, Q, R, \cdots$ be n observations forming a set, then, since they have all the same m.e.,

$$(\text{m.e. of } Z)^2 = n(\text{m.e. of one observation})^2, \text{ and}$$
$$\text{m.e. of } Z = \sqrt{n}(\text{m.e. of one observation}).$$

The a.m. of $P, Q, R, \cdots$ is of course $\dfrac{Z}{n}$;

$$\therefore \text{ m.e. of the a.m.} = \frac{\text{m.e. of one observation}}{\sqrt{n}}.$$

These results obviously apply to the p.e., e.m.s., and C, of a set of fallible results, thus:

$$\begin{array}{l}\text{m.e.}\\ \text{p.e.}\\ \text{e.m.s.}\\ C\end{array}\text{ of the A.M.} = \frac{\begin{array}{l}\text{m.e.}\\ \text{p.e.}\\ \text{e.m.s.}\\ C\end{array}\text{ of a single observation}}{\sqrt{\text{No. of observations in a set.}}}$$

Thus; four observations will halve the m.e. of the probable value of a measured quantity; ten observations will divide it by about 3; a hundred observations will reduce the m.e. to one-tenth of its original figure; whilst, to reach one more significant figure, ten thousand observations are required. Another useful consequence of this deduction is as follows: Let m.e._1, $\text{m.e.}_2, \cdots, \text{m.e.}_n$ represent the mean errors of observations due to a number of independent systematic errors of adjustment, each acting alone; then suppose there is no more reason to assume that any one maladjustment will be positive rather than negative, and that the product terms of these causes acting together are negligible, it follows that the mean error of observation, due to all errors of adjustment acting together, is:

$$\sqrt{\text{m.e.}_1{}^2+\text{m.e.}_2{}^2+\cdots+\text{m.e.}_n{}^2}.$$

Thus, if the e.m.s. due to ten independent causes acting alone are numerically equal to

1, 2, 2, 1, 4, 3, 5, 7, 6, 1

the m.e. of the final error of observation is only about 12; although there is some risk of an error amounting to their sum, namely 32.

Since the m.e. due to any one maladjustment is equally likely to be positive or negative, the risk of all the partial errors having the same sign is $(\frac{1}{2})^{n-1}$. In this case the probability is 1 in 512. This theorem is of practical use to the designer in assisting him to determine the limits of accuracy of manufacture and

> adjustment required to achieve a given probable accuracy. Thus, given the working tolerances contemplated for the various components and the adjustments (whether maker's or observer's), he can work out the errors due to the extreme values of these tolerances acting alone and apply the above calculation.
>
> Two conditions must be observed: The product terms of the maladjustments must be negligible, and the errors must be independent. This method would fail hopelessly in the consideration of backlash in a number of couples forming one train, for the whole combined backlash is taken up together and in the same sense.

This very brief description does not begin to cover those parts of the theory of errors required by the practical scientist, but is, I believe, adequate for most purposes of instrument design. The deductions from the theory of errors given above are logically interconnected and any one could be deduced from the others, subject to the fact that certain approximations are made in the mathematics which, however, are not serious.

No mention has been made of work by Professor Karl Pearson and others, on the methods of analyzing a sample "population" to evaluate the degree to which the relevant probability curve may be "skew". This work is obviously important to the practical scientist in many fields, but has not much application to the rectification of errors of measurement, for the number of repetitions of any one measurement are usually far too small to investigate the actual probability laws involved; all that can be done is to assume some standard normal law and hope for the best.

Discussion

It is not always easy to discover exactly what application is intended by writers on the theory of unbiased errors; they usually describe these errors as "errors of observation" and imply that all such errors are unbiased. The word "observation" is rarely defined and seems to be confined, at least by some writers, to the human part of a measurement; the presumption being that the mechanical elements are only subject to systematic error, which will not affect the precision of a set of repeated readings. This position, though often implied, is probably not seriously maintained by any one who has had practical experience of instruments; and, since in the presence of scatter in a set of readings, it is usually quite impossible to disentangle those effects due to the human and mechanical factors, the "Law of Errors" is in fact applied to the whole scatter irrespective of origin.

The whole scatter of a set of readings is thus assumed to obey this Law of Errors. This very large assumption, if true, requires demonstration, and two lines of argument are commonly adduced.

1. The law follows from the theory of probability as applied to certain assumptions, already stated, as to the number and character of partial errors. Accepting the applicability [1] to problems of this kind, it is sometimes urged that the assumptions with regard to partial errors are in line with practical experience. I endeavor to throw doubt on this proposition in subsequent chapters and, to their credit, most writers find it advisable to reinforce this argument with a second, namely:
2. That "Whatever may be thought of the process by which this formula has been obtained, it will scarcely be doubted by any one that the result is entirely in accordance with our general ideas of the frequency of errors"; I quote from Airy [2] who is defending the frequency formula already given. In the next paragraph,

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[1] Keynes, *A Treatise on Probability*. I am in some doubt after reading this work as to whether a numerical value could always be assigned to the probability of an error of a given size; or whether in some cases, there is no such thing as a *definite* probability, in this sense.

[2] *Theory of Errors of Observations*, paragraph 22.

> after indicating the form of the probability curve, Airy continues: "This is precisely the kind of law which we should a priori have expected the Frequency of Error to follow; and which, without such an investigation as Laplace's, we might have assumed generally; and for which, having assumed a general form, we might have searched an algebraical law".

In a general way, the probability curve (Fig. 18) undoubtedly does correspond with experience. For large errors are less frequent than small ones, and very large errors (in comparison with a likely error) are practically nonexistent; Table I shows this very clearly. Beyond this I can see nothing self-evident about the curve.

In comparison with zero errors which have a relative frequency of unity, errors equal to the modulus have a frequency of about one-third, and errors of three times C occur about once in eight thousand times; whilst an error of five C, or about nine times the m.e. should occur once in about one hundred thousand million times. Thus, if a thousand scientists each took an observation every five minutes for a thousand years probably only one would once make so gross an error due to unbiased causes. When it is remembered that this unique disaster only consists in making an error nine times as large as the average run of the errors, grave doubt is thrown on the practical value of this law for assessing the chances of very exceptional events. We shall return to this matter later.

However, the vast bulk of errors clearly lie between $\epsilon = \pm 2C$, and the shape of the curve tells us that the observation is more likely to be correct than to have any other particular error; and this at least agrees with common sense. We see that very small errors are nearly as likely as zero errors but that the frequency rapidly descends until the error grows to about twice the mean (or average) size of error; after that errors of large size very rapidly become negligibly rare. It is true that in practice errors of above a given size are not unlikely, they are quite impossible. Thus, in the Hilger comparator the field of view of the microscope may be perhaps 0.2 inch if a low power is used. If the mark set on is more than 0.1 inch from the optical axis it will not be seen and cannot be used. This is not a serious defect in the theory, for the limit of error in the case of the comparator would represent perhaps an error, $\epsilon = 20$ or $30C$, and its probability is so nearly 0 that the difference is of no moment.

The obvious way to show that the laws of probability are applicable to errors of observation is, of course, to apply them; and constant reference is made to the overwhelming character of this evidence; but it is usually not quoted, or when quoted, it does not apply to measurement.

This faith in the mass of evidence round the corner is partly due to a confusion as to what it is that requires proof. Evidence as to the tossing of coins and games of chance generally may afford evidence as to the validity of a theory of probability, but it cannot show that the theory has been correctly applied to observation and measurement. It is tacitly assumed that, if the accepted Law of Probability can be shown to be applicable to any given class of phenomena, it thereby follows that it applies in the same way to all phenomena to which laws of probability are in general applicable. Actually the accepted law has only been mathematically applied to those errors which arise from a very restricted type of partial error, and what little is known about actual partial errors rather throws doubt than otherwise on the applicability of the assumptions.

It seems, therefore, that each type of phenomenon must be independently examined before a frequency curve can be properly ascribed to it. It is not easy to say what constitutes a "type" in this connection but some broad distinctions can be made.

In the first place, statistical matter falls into two main groups: series which represent deviations from a mean determined by some persistent restoring agency, and series of deviations from a mean which has a purely statistical existence. As examples of the first class: the successive positions of rest of a damped pendulum repeatedly disturbed—here the restoring force is gravity, and the mean position is determined by geometrical considerations; the position of a travelling microscope repeatedly sighted on to a fixed mark—the mean position is determined by the mark, and the restoring agency is the human appreciation of asymmetry when the graticule is incorrectly on the mark. In the second class there is no restoring agency, and the resultant mean has no significance with regard to a single event. Thus, on one throw of a coin there is no sense to be made of the statement "The mean to which this throw is approximating is one-half heads". The penny is not urged to come down one-half heads; it is incapable of ever doing so. The word "mean" in this connection is merely shorthand for the statement

$$\frac{\Sigma_n \text{ observations}}{n};$$

and can be given no other significance.

This applies to all averages in which no effort (conscious or unconscious) is being made towards a true repetition of the event, and renders this whole class of events quite useless as evidence with regard to the law of frequency of errors applicable to observations of the other type. This is unfortunate, for coins and Monte Carlo between them provide some of the best evidence with regard to the accepted law of probability; though it may be pointed out that neither of these have proved good friends. According to Keynes, a Swiss scientist tossed dice 280,000 times, and the result shows the dice to have been biased; but this is a circular argument. Monte Carlo shows a tendency, according to Professor Pearson, of favoring long runs unduly; whilst Dr. Karl Marbe came to the opposite conclusion. Statistics, with regard to population and to gas laws, may refer to events which tend to possess controlling agencies defining the means, but the laws connected with these agencies are so unlike those involved in careful mensuration that this class of statistics will not help.

The importance of this matter to the designer lies largely in the fact that, in so far as the theory of probability is applicable to errors of observation, these errors can be diminished by repeated observations on the part of the observer; thus increasing the accuracy of the measurements obtained by a given instrument. The degree to which this method can be pushed depends partly on the design of the instrument, in that biased errors must be kept below the significant level of this greater accuracy, or changed in character so as to lose their bias.

In the next chapter the character of short period errors is investigated, and an estimate is formed as to the extent to which the classical "theory of probability" is applicable.

CHAPTER IV

SHORT PERIOD ERRORS (DISTURBED ELEMENTS)

CAUSES OF DISTURBANCE

There is a large class of disturbing agencies which results in short period disturbances of elements, and their character and effect will be considered; they usually lead to short period errors.

Every observation is surrounded by an incredible number of causes of disturbance and therefore possible error. The whole universe is interconnected; our earth is influenced by heat from the fixed stars, and a man stamping his foot in Hong Kong shakes a laboratory in Europe, though neither effect is impressive. This interconnection assures an ample supply of causes of disturbance to any observation, but they are of very different importance.

Probably the most important disturbing causes lie in the instrument or the observer; specks of dust causing sliding parts to move away from their mean position, or resulting in irregular friction; unsteadiness in the observer's hand or eye, or in his judgment; and so forth. The room in which the observation takes place may well be the seat of important causes of error; draughts, both by creating pressure and temperature, and, in special cases, humidity or other differences; the other man who coughs or talks or shakes the room; the door that slams; and many other things.

Further afield are such disturbances as traffic (not one cause but as many as there are vehicles); electrical disturbances due to machines being switched on or off (e.g., the reception of wireless signals); the indefinite noises of a town—these are really pressure variations in the air and solid matter; a sufficiently delicate instrument will show the whole earth to be in a continual state of tremor; vibration and strain due to land tides travelling round the equator at about 1,000 miles an hour; vibrations of radiated heat due to the passage of clouds; effects due to wind and rain; and many more. I remember trying to level an apparatus in a well built laboratory in the country, with a spirit level reading to seconds of angle, working at a time when no one else was in the building; I found to my dismay that the whole building appeared to be rocking slightly with a period of a minute or two; this seemed to be too slow to be the natural period of the structure and suggested a disturbance in the neighboring surface of the earth.

Two things suggest themselves. First, as a general rule, a cause of error will be more important if seated close to the observation than if located at a distance; secondly, the number of causes increases rapidly with their distance from the place of observation. Thus, there are more men stamping their feet in China than in the observer's room, but the former are individually and collectively less important.

These remarks are subject to frequent and obvious exceptions, but nevertheless convey a general truth. The inference is that causes of error seated close to the observation are not only more destructive of accuracy but are probably too few in number to obey any laws of probability; whilst the reverse statements are true of more distant causes. Hence, it is to be expected that a carefully designed instrument, which has been guarded against its own possible defects and from immediate influences, will not only be subject to smaller unbiased disturbances than a badly designed instrument, but that these will be more amenable to reduction by mathematical treatment. This is exactly what is found in practice, for it is common experience that very little can be done to improve the accuracy of a poor instrument by repeated readings, but that a good instrument sometimes warrants a fairly large repetition.

BIAS

The independence of causes of disturbances, especially the smaller ones seated at some distance from an observation, is fairly obvious; a lorry three roads off will not be connected with the local power station; and, finally, these causes are likely to be unbiased in the sense that, at any one instant, it is a level chance as to whether the disturbance propagated in the direction of the observation be of one sign or the other; these disturbances usually take the form of pulsations of matter, or ether, and so change sign frequently. Thus, if a few large causes of disturbance, often seated near an observation, are guarded against, the remaining multitude of small causes do seem to have the characteristics postulated for causes of partial unbiased errors as described in the last chapter.

It is usually assumed, more or less tacitly, that causes of a given type lead to partial errors of more or less the same type, and this is not necessarily true. In particular, it is commonly untrue to suppose that an unbiased cause leads to an unbiased partial error.

As a matter of fact, no one has ever experienced a partial error, and, in general, an error of observation is the resultant, not the sum of the partial errors ruling at any one instant; it will, therefore, be more useful to consider the errors due to causes of disturbance from a slightly different angle.

An observation is a highly localized event; it concerns a few elements commencing with an object and ending with the mind of an observer, and is often contained within a radius of five or six feet,[1] and, however various and scattered may be the causes of disturbance, they can, in fact, only disturb by acting on these few elements roughly situated in one place. Moreover, of the elements available for disturbance in any one observation, some one or two will outweigh all the others in importance and liability to derangement. Suppose the resultant disturbance (due to all disturbing causes) in the immediate neighborhood of a goniometer be a vibration; then let *A*, Fig. 19, represent the rotating member of a goniometer, centred at *C*, and with its *CG* at any other place as shown. If this instrument be subject to a rapid vibration of small amplitude, since clamps are imperfectly rigid, and often needlessly so, *A* will vibrate through a very small angle with respect to its stand. The result will be that the image of the scribed line on the circle, as viewed through the reading microscope, will appear a trifle wider than its dimensions really warrant. This will probably not affect the accuracy seriously provided the excursions of *A* are symmetrical, or unbiased, with respect to its position of rest. Suppose the fine motion consists of a tangent screw spring loaded as shown diagrammatically; then, clockwise excursions of *A* are opposed by the screw, whilst the reverse rotation is only opposed by the far more yielding spring, with the natural result that the mean position of *A* under vibration will be somewhat displaced from its position of rest in an anticlockwise position, irrespective of the fact that the vibration of the room and table may be completely unbiased. The fine motion has "rectified" the original unbiased disturbance, to borrow a term from another science, and caused a quasi permanent or erratic, and biased, error. The solution in this case is, of course, to design such parts as *A* in balance about their centres of rotation and to provide

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[1] An extreme exception is an astronomer observing a star.

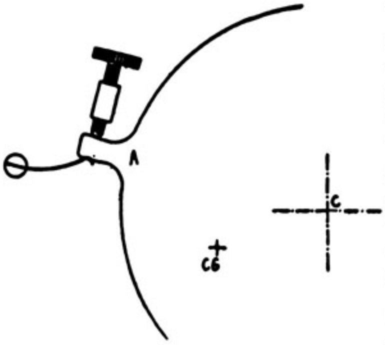

FIG. 19.

efficient clamps. But this illustration only provides a gross example of a very general state of affairs; namely, after taking all care to preserve such symmetry as is possible and desirable in an instrument, it is impossible to eliminate the danger that errors due to an unbiased cause will be given a bias by the instrument itself; and all similar types of errors will be given a bias in the same sense.

In general, then, it is not sufficient that the resultant disturbance surrounding some element shall be unbiased in order that the short period errors of observation shall obey the "Law of Errors"; it is equally necessary that the element shall put up an unbiased resistance to this disturbance; that is, the restoring agencies in the element must be symmetrical about their position of zero moment. The last example, and many others known to every one, are illustrations of the fact that this condition is often grossly violated.

Relation between Disturbance and Error

Another condition is needed in order that short period errors of observation shall obey the "Law of Errors". Assuming that the resultant disturbance surrounding an element obeys the law, and waiving any question of bias, it is still necessary for the error of observation to vary in direct proportion to the size of the resultant disturbance. This involves two things: first, that the size of the error shall be in proportion to the disturbance of the element—this is not necessarily the case, but in practice is very usual; secondly, that the disturbance of the element shall be in proportion to the resultant disturbance surrounding it; this implies that the restoring agencies in an element shall have a moment which varies directly as its disturbance, and this is very frequently not the case. There is, however, one class of element in which both the above conditions are very often satisfied, but this is not the kind of element which usually gives trouble in a well designed instrument; I refer to the momentary elastic deformation of some one component or part of an instrument.

If a weight is hung on a spring, the pointer tends to some scale reading X, and if disturbed, the restoring moment is KD; where D is the departure of the pointer, and K is a constant of the spring. This is typical not only of intentionally elastic members, but of many elements formed of a single piece of metal. In the presence of friction, however, it will be shown later that this relation between the disturbance of an element and its restoring moment leads to an incomplete element, and to erratic errors.

RELATION BETWEEN DISTURBING AND RESTORING AGENCIES; EXAMPLES

One or two other examples of the relation between the disturbance of an element and the moment of its restoring agency are given as typical of many other elements. In practice, it is often extremely difficult to state the precise relationship between these two quantities, but a shot at the sort of relation existing can often be made by inspection. Fig. 20 shows a nut engaging a leading screw; and, for clearness, only one turn of the thread is drawn. This nut is supposed to be attached to the saddle forming part of a travelling microscope. The reading scale is attached to the screw, and a relation is assumed between the position of the nut along the screw and the number of turns performed by the screw; the accuracy of observation depends, among other things, on the constancy of this relation. In the ideal case the thread of the screw will be exactly the same size as the space available between two turns of the thread in the nut. That is, faces A and a, B and b, will be in contact over their whole surfaces and there will be no pressures involved and

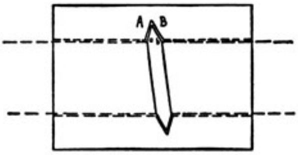

FIG. 20.

no distortion. Similarly the microscope saddle and slide will be a perfect fit, sliding without friction. In practice, if the saddle and slide have the slightest slackness, the instrument is valueless; consequently, these parts are assembled somewhat tight; their resistance to motion is far from negligible, and constitutes a disturbing cause with respect to the nut and screw. As explained in a previous chapter, such measurements are always corrected for zero, and, in consequence, the sliding friction of the saddle is immaterial provided it is constant, and provided the "cut" is always performed with the nut travelling in the same direction. In practice nothing is so liable to sudden and irregular fluctuations as the coefficient of friction, nor is this danger diminished in the very usual case where the slide is unprotected from dust. If the slide is not oiled and cared for, it becomes coated with a thin film of semidried greasy matter choked with foreign matter including minute particles of metal, whilst even if it is wiped and oiled before use, absolute constancy cannot be expected. Apart from dust and other irregularities, the coefficient of friction between two sliding surfaces is only very approximately independent of the rate of their relative motion, and not even approximately so at just those speeds employed in a careful observation; for it is well known that when two sliding surfaces are not in relative motion, considerably more force is required to start motion than to maintain it.

This is not a question of acceleration; the surfaces become in some manner locked together and have to be unstuck; the force required to effect this is called "stiction" to differentiate it from true sliding friction. A little consideration will show that friction and stiction cannot be quite so sharply differentiated as this suggests. No two bodies ever are or can be expected to be in a state of relative rest in the last analysis. Both consist of highly mobile molecules, and minute movements must be occurring incessantly. What movements are compatible with a state of stiction, and when does true friction appear? Clearly the change from stiction to friction cannot be abrupt, and the real facts must be something like this: if one body be slid over the other with a diminishing speed, the coefficient of friction will be found roughly constant over a fairly wide range of speed; but, when the speed has become very low, the friction will rise rapidly as the speed diminishes, reaching a maximum as the speed falls to zero. Now, when a delicate cut is being made, at the last instant the slide is frequently being moved at a speed well below 50 μ per second, or less than 3 mm. per minute; and there is reason to suppose that at these speeds the coefficient of friction is a highly variable quantity.

For these reasons the forces opposing the motion of the nut are variable and, since the measurement of a length will be of the form,

$$L = l_2 - l_1,$$

any variation of this force when the two cuts are made will cause an error; unless, of course, the relationship between the nut and screw can be made substantially independent of this disturbing cause. From the general design of such instruments, if F_1 and F_2 be the two frictional forces opposing the nut when the cuts at l_1 and l_2 are performed, the disturbance,

$$D = \frac{F_2 - F_1}{\text{Resistance of Restoring Agency}}.$$

The restoring agency is the resistance of the screw and nut threads to deformation; if the modulus for this elastic resistance be K_1 for the steel screw, and K_2 for the nut (usually a softer metal),

$$\epsilon = \frac{F_2 - F_1}{AK_1} + \frac{F_2 - F_1}{BK_2}$$

$$= \frac{(AK_1 + BK_2)(F_2 - F_1)}{AB \cdot K_1 K_2}. \qquad \text{XXIX}$$

For, in this case,

$$\epsilon = D.$$

A and B are constants of the screw and nut threads respectively, depending on the length of thread engaged and their geometric form; usually, but not necessarily, $A = B$.

In this case, the error of observation varies with the resultant disturbance, which is the difference between a friction force on two occasions. Though, in actual fact, the relationship is not quite so simple, for the nut and screw are partly separated by air and oil, not to mention other substances, and the restoring force, or K, of this layer will have a more complicated relation to D.

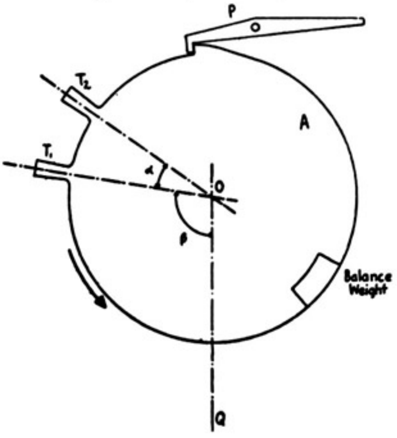

FIG. 21.

To take another example, it is sometimes required to control some very short interval between two events, and this may be done by utilizing the acceleration of a given mass under the influence of a suitable force. I had occasion to use such a device which gave very constant results. Fig. 21 represents a simplified form of the device in question. For purposes of this explanation it will be assumed that a narrow beam of light normal to the paper crosses the line OQ close to the periphery of the rotatable disc A. The problem is to interrupt this beam twice within a very short interval of time, and this is accomplished by means of the two teeth, T_1 and T_2, fast to A, and separated by the angle α. The disc A is rotated by means of a spring in the anticlockwise direction as soon as the pawl P is released. The disc accelerates through the angle β before T_1 interrupts the beam, and, continuing to rotate through the additional angle α, causes the beam to be interrupted again by T_2.

Let the moment of the spring, supposed constant, be M_S, where $M_S = F\tau_1$, F being the force exerted by the spring, and τ_1 its radius of action.

And suppose the moment of the disc A, together with catch c both teeth and balance weight, to be M_A, where,

$$M_A = W\tau_2.$$

W being the weight of A, and τ_2 its radius of gyration.

Then, the angular acceleration of A is,

$$\frac{M_S}{M_A} \text{ radians per sec. per sec.}$$

F being expressed in absolute units.

If t be the time interval between the two interruptions of the beam, then, by the laws of dynamics,

$$t = \sqrt{\frac{2M_A}{M_S}}(\sqrt{\alpha+\beta} - \sqrt{\beta}). \qquad \text{XXX}$$

t could be adjusted by controlling either the angles or, more conveniently, the moments; but the essential feature of the device lay in the *constancy* of t. The two characteristics most liable to changes are the force of the spring, and the friction forces connected with the bearings for A. The acceleration required was high compared to g, and the spindle had plain journal bearings. Any change in the spring would be more or less permanent over short periods, and so lead to systematic errors whilst the friction force, though liable to fairly rapid fluctuations, can be included in Equation XXX if $M_s - M_f$ be taken to be the difference between the moment of the spring and that of the friction and substituted for M_s above. Since all the other terms are highly constant and need not concern us, we may write,

$$t=\frac{K}{\sqrt{M_s-M_f}}; \qquad \text{XXXI}$$

where K is a constant of the instrument.

The spring was well designed and tempered, aged, and working within reasonable limits of stress, and so was not liable to day by day alterations (apart possibly from a small temperature effect).

Hence, concentrating attention on the friction term, let M_f change by some amount ΔM_f, then;

$$t_1=\frac{K}{\sqrt{M_s-(M_f+\Delta M_f)}};$$

and

$$\epsilon=t_1-t=K\frac{\sqrt{M_s-M_f}-\sqrt{M_s-(M_f+\Delta M_f)}}{\sqrt{M_s-M_f}\cdot\sqrt{M_s-(M_f+\Delta M_f)}}\cdot \qquad \text{XXXII}$$

Thus, ϵ becomes infinite when,

$$M_s-(M_f+\Delta M_f)=0.$$

This is obvious, since, when the friction moment equals that of the force, no movement will result; and this is also the limiting value for the friction moment. This method of measuring, or controlling, a time interval can hardly be of much importance unless, for likely values of μ, the change in frictional moment, ΔM_f, is small compared to the net driving moment, M_s-M_f. Assuming this, an approximate value of ϵ is obtained by differentiating t with respect to the net driving moment in XXXI; thus,

$$dt=\epsilon=\frac{K}{2(M_s-M_f)\sqrt{M_s-M_f}}\cdot dM_f. \qquad \text{XXXIII}$$

It is seen how insensitive t is to small variations of M_f. Thus, if $M_s=100$, $M_f=10$, and $\Delta M_f=5$, $\epsilon=0.003K$ nearly;

or, in other words, a change of over 5% in the net driving moment causes an error of under $\frac{1}{3}$% in the time interval. Nor is this a particularly favorable example, since the friction moment has been taken as 10% that of the gross driving moment, and the former has been supposed liable to a disturbance of 50%.

A change in the friction moment, or simply in μ, seeing that the radius of action is constant, is the disturbing cause; and its moment has been kept relatively small by swamping it with a large driving moment, and seeking an effect with which it has an insensitive relation (as shown by the $\sqrt{}$ sign in XXI). This is an example of good design, but some uncertainty exists as to the restoring agency; what, for instance, tends to restore the value of μ to its original figure when once it has changed? Clearly its lower limit is zero, and, strictly speaking, there is no upper limit to μ, though, given good bearings protected from dirt and reasonably oiled, in practice some value can be found which is not likely to be passed; equally as a matter of practice some lower figure, above zero, will be exceeded. If μ_1 and μ_2 be the practical lower and upper limits, respectively, can it be said that any agency is continually urging μ towards some particular value, μ_x, say $(\mu_1 < \mu_x < \mu_2)$? This is very doubtful, although it is unlikely that, if μ_1 and μ_2 are not overstepped, μ will often have values very close to these practical limits; but there may well be some closer limits μ_m and μ_n such that,

$$\mu_1 < \mu_m < \mu_n < \mu_2;$$

and between μ_m and μ_n the restoring agency may be excessively weak, or even nonexistent. This seems all the more probable when it is remembered that μ depends on surface conditions which do not necessarily tend to revert to their previous state when disturbed; and, moreover, it is a matter of common experience that friction is one of the most unstable characteristics of any mechanism; it is this rather than its absolute size which makes friction such an enemy to accuracy, and causes a whole host of friction devices to be so unreliable.

It may be noted that a square law relationship is very common in nature between two dependent variables. If one of these variables, x, be a disturbing cause, and the other, y, one of the "constants" involved in an observation, when,

$$y \propto x^2$$

the chances of obtaining a satisfactory observation are very favorable. This applies to such instruments as anemometers, ships' logs, centrifugal governors, etc.

In the example above, not only does the disturbance of the element *not* vary with the resultant disturbance surrounding it, but it is very questionable whether the latter obeys the "Law of Errors" in any case.

Another rather general source of error arises in connection with instruments under vibration, or whose indicating device is purposely damped, with a view to "meaning" the indications of a hunting or oscillating mechanism. In the case of a damped pointer, the damping frequently consists of a copper disc rotating between the jaws of a magnet, or, alternatively, liquid or air damping. In the first case, the damping force varies in proportion to the velocity of the pointer; in the latter cases, this is only true for very small velocities, the force varying as the square of higher velocities. If the oscillations are in every way symmetrical about their means, the pointer will show this mean (i.e., oscillate very slightly about it) whether the damping obeys a first or second power law. In the presence of asymmetrical oscillations these two laws lead to different mean positions of the pointer. Which, if either, is correct depends on how the "mean" is interpreted, but obviously both systems of damping cannot be correct for any given interpretation. Mr. E. A. Griffiths [1] states that first power damping is correct, and his argument amounts to proving that it effectively integrates the fluctuations of an undamped member; at any rate for simple types of fluctuations. This is evidently what is required, e.g., to determine the total quantity of electricity (liquid or gas, etc.) passed between two times, knowing the rate of flow at every moment; but not all observations are of this nature. Suppose, for example, it is desired to obtain the midposition between the two extremes of fluctuations, neither form of damping considered would be correct. The point to notice, however, is that, if any rapid fluctuations exist in the quantity to be measured, the damping of the record (whether intentional or otherwise) will in general cause an error which is a complicated function of the amplitude, frequency, form, and asymmetry of the fluctuations. In such cases, the restoring agency consists of whatever means are provided tending to make the pointer follow the measured quantity, but what is really assumed may be that the pointer reading bears some relationship to this quantity (e.g., integrates it, or does something else). If the pointer is caused to hunt the right sort of mean (or if the means coincide, as in the case of symmetrical oscillations) no error results; but in any other circumstances, the resultant disturbance—an oscillation—has a moment depending on its size and form, etc. Even if the oscillations are themselves accidental variations from a symmetrical form, the departures of the pointer from the required mean are unlikely to obey the Law of Errors; whilst if the oscillations tend to some definite asymmetry, the error will be biased, though hardly capable of treatment as a systematic error unless the strength and character of the oscillations can be controlled or ascertained. This is a type of erratic error.

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[1] *Engineering Instruments and Meters*, Chapter V.

MENTAL RESTORING AGENCIES

As already pointed out, not all restoring agencies are material, and it becomes important to consider whether these nonmaterial agencies have a "moment" which can be said to vary with the disturbance of their elements.

The sense most commonly used by the observer when making an observation is sight, and there is some evidence according to Dr. French and others that the determination of the upper and lower threshold limits is more precise than that of the position of correct setting in the case of a null setting or "cut". The two threshold limits are the greatest plus and minus errors in a setting which will pass unnoticed. Systems have been evolved in which the thresholds are obtained and their mean recorded, the assumption being that the threshold limits are equally spaced on both sides of the "cut". This is sometimes done by, more or less consciously, noting the two positions of the controlling hand, and moving the control over to the midposition. This is an interesting example in which one sense estimates the symmetry of a range of values about the mean, and leaves it to another sense to bisect this range. Assuming that sight alone is used, the hand being only an obedient servant, it is not easy to see what form of indication the eye conveys to the mind of the observer.

In the case of normally good vision, the eye has the faculty of resolving two points separated by not less than about 50″ of angle, but it is well known that skilled observers can set well designed symmetrical cuts with a m.e. not exceeding 5″ to 10″ of angle at the eye, depending on circumstances. That this is not altogether a "hand and eye" business is evidenced by the fact that it applies to such instruments as range finders, where there is little time to effect such a combination, or to determine the threshold limits, consciously or otherwise. This amounts to the following statement: if a skilled observer is shown a setting or "cut" already performed, and is asked to judge its truth without touching the control, he will probably judge it correct provided the error does not exceed about 50″ at the eye; the same observer will, however, fairly consistently perform the same cut with errors not exceeding 5″ to 10″. The inference is that the mind receives a number of sensations of different character, and then performs some mental act of synthesis, the outcome of which is an order to the controlling hand.

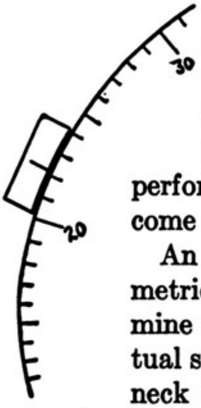

FIG. 22.

An instance of a different kind is the unsymmetrical setting where the observer has to determine readings as shown in Fig. 22. Here the actual setting is performed elsewhere, and the bottle neck is supposed to be the subsequent reading; this is typical of all direct measurements of the type in which a scale is laid along a length to be evaluated, and occurs occasionally in other types of measurements. Since the observer is presented with a fait accomplis, the limit of resolution of 50″ at the eye is probably one factor in the error; but the case is not so simple as this, for no two points requiring resolution exist, and the observer is required to perform some sort of geometric construction which is superimposed on the visible field. At any rate some new factor clearly enters here with, as usual, its unknown law of error.

FIG. 23.

Yet a third type of setting is the "symmetrical" cut in which a fine graticule line has to be superimposed on a fuzzy and usually unsymmetrical coarse line. Figure 23 illustrates this, and is typical of a setting on a photographic line, taken off the axis of the camera lens, and viewed, of course, under considerable magnification. Here a real difficulty arises as to whether the graticule should "cut" the middle of the area, or the centre of gravity of its density, or merely the densest part. An error in this judgment will lead to a systematic error, and need not concern us here; the correct answer is, of course, dependent on the nature of the unsymmetrical aberrations with which oblique pencils are necessarily burdened.

Suppose the observer has correctly determined to set on the c.g. of the density, what type of mental and sensory activities are in play, and what are their laws of error? This type of setting, very common in some types of work, leads to a curious result which throws some light on the mental processes involved. Even when the setting is as bad as shown in Fig. 23, a surprisingly high precision can be obtained for a set of perhaps 20 readings taken at a sitting. If the observer is then interrupted, speaks to some one, or has lunch, etc., and repeats the set, he will obtain a similar precision; but the means of the two sets may well be separated by an interval at least five to ten times the m.e. of either set. Similarly two observers performing the same operations may have the means of their sets so separated.

This suggests that the difficult operation of determining the c.g. of the density is performed only once in a set of readings, namely, on the first reading, and that unconsciously some subsidiary mark or characteristic is chosen corresponding to the first setting, and used on subsequent occasions; this characteristic may be simply a memory of the general impression of the cut as made. If any interruption occurs, this characteristic is forgotten and a new judgment is required.[1] A further judgment is always used on such a cut, namely, the setting of the graticule on a rough area is "meaned" up and down the length to eliminate local peculiarities of the area or "line" set on.

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[1] I am aware that Fig. 23 represents a case of gross overmagnification, and that more accurate results would follow a dropping of the power; this would reduce, but not eliminate, the effects discussed. The drawing is exaggerated for the sake of clarity, but my statement that the separation of the means may exceed the m.e.s. by five or ten times is quite possible in a well conducted experiment.

It is not always realized what a very large part mental judgments play in the interpretation of physical stimuli. This is clearly shown by the well attested fact that the skill of an observer is far more dependent on his experience and mental stability than on his physical acuity. As an example of this, I have often performed measurements of the type described together with an optical physicist of great experience in such work. We both use our right eye, and mine is without doubt superior to his as an optical instrument; nevertheless, he almost invariably achieves a precision some 20% or 30% greater than I, and is less liable to erratic errors due to change of judgment. A mental state of peace and self-control is definitely sought, when performing delicate measurements, which has been described by this observer as the "precise frame of mind".

Discussion

The object of enumerating these various examples of observation, in which the human presents the weakest link in the chain, is to illustrate the great variety of human characteristics involved, both physiological and psychological; and to emphasize the improbability of each of these characteristics having the same law of error.

It is stated by Fechner that sensation varies as the logarithm of the stimulus, and, according to Keynes, if this is so it follows that the a.m. will not lead to the most probable value, and a further corollary would seem to be that the accepted exponential Law of Error is not applicable. Later work by physiologists has thrown great doubt on Fechner's Law. It has been shown that, for very weak stimuli, sensation varies at a faster rate than the logarithm of the stimulus; and that the exact opposite is true for very strong stimuli. In the case of a stimulus of moderate intensity, the logarithmic relation represents a good approximation in most cases, but even here it can only be taken as an approximate statement.

We are principally interested in the relation between very weak stimuli and the resulting sensations, and here the ratio between the two varies rapidly with the strength of either, and is very different for each type of stimulus. Consequently, it is quite impossible that a standard law of error should apply rigidly to errors depending on human elements. The most that can be hoped is that this law should represent a rough average approximation to the truth; and actual evidence as to the scatter of observations, where the human factor represents the bottle neck, does not always suggest even this moderate correspondence.

It is true that human restoring agencies can only work through some physical means, but these means are not the true agencies, since nothing is demanded of them except that they shall obey the mind, the mean position of the two links being determined by the *opinion* of the observer. The laws connecting sensation with disturbance are largely unknown, but the great variety of mental activity connected with this sensation does suggest that it cannot be self-evident that sensation and disturbance always vary in the same proportion; thus, material and mental restoring agencies seem to be in line in this respect.

Summary

Three separate propositions have been demonstrated in this chapter.

1. The resultant disturbance surrounding an observation does not necessarily obey the accepted normal Law of Errors; but that when one or two major causes of such disturbance are removed or guarded against the remainder frequently may obey this law approximately.
2. The short period errors which follow momentary dis-

turbances are frequently biased even though the latter may not be so.

3. When short period errors are unbiased they do not necessarily obey the Law of Errors, even though the antecedent disturbances may obey this law.

In considering the application of the Law of Errors to practical problems, Propositions 2 and 3 are of very different importance; for, if the short period errors (i.e., scatter) of a set of similar observations are liable to bias, it cannot be of much use to apply the law in any way. In such a case, the a.m. of a set has very little more significance than any one reading.

On the other hand, provided the short period errors are unbiased, as they often approximately are, it is likely that the accepted probability curve for the frequency of errors is some rough approximation to the real curve for the observations in question.[1]

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[1] Keynes in his *Treatise on Probablity,* Chapter XVII, proves that, given certain assumptions, the arithmetic mean gives the most probable value of the quantity measured for any law of error of a given general type, of which the usual normal law is a particular example. This is not the only type which is reasonably possible in practice and Keynes shows that other types of laws lead to other functions of the observed values. Later he remarks that " . . . if any such law of sensation, as that enunciated by Fechner, is true (i.e., that sensation varies as the logarithm of the stimulus), the arithmetic mean must break down as a practical rule in all cases where human sensation is part of the instrument by means of which the observations are recorded".

For the practical scientist this does not quite go to the heart of the matter; for not only must an acceptable function of the observed values give a probable value of the quantity measured, but it must do so with the least possible risk; that is, with the least possible likelihood of an occasional failure.

Thus, suppose a quantity is measured by 10 separate observers each taking several readings, then let the actual errors of the arithmetic means of these 10 sets be;

$$1, -1, 0, 0, -2, 0, +1, 0, 0, -8.$$

The average error of the arithmetic mean in this instance is 1.3. Now let the following errors refer to any other method of arriving at the probable value of the quantity from identically the same sets of observations;

$$-2, 1, 2, 1, -2, 1, 2, -1.$$

The average error of this method of determination is 1.5, which is about 15% worse than 1.3. On the other hand, the worst error in the latter case is 2 as against 8 when using the arithmetic mean. If this were typical of the two

The very act of taking the m.e. automatically ensures that the real and assumed curves coincide at three places; namely, when $\epsilon = 0$, and when $\epsilon = \pm \text{m.e.}$ The two curves cannot well get very far apart between these values, and for a short distance beyond them, and this includes a large proportion of the likely errors. What it does not include are the unusually large errors that sometimes occur, and there is no reason to suppose that the Law of Errors even approximately shows their likely frequency. This is significant, for the almost universal experience of practical scientists is that "huge errors" occur with far more than their expected frequency in sets of observations; and two reasons, I think, account for this: the first has just been given, and the second is due to the fact that erratic errors due to incomplete elements are interposed with short period errors; but this is discussed in the next chapter.

The outcome of the above reasoning and experience seems to be that, in a well designed instrument, some use is to be made of the Law of Errors; for the a.m. of a set of observations is likely to be more reliable than an individual observation (apart from diminishing the risk of a mistake), and the various expressions of precision (m.e., p.e., e.m.s.,

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methods, the second would be preferable for almost all purposes; there is not much point in attaining an accuracy which cannot be reasonably depended on.

Now the arithmetic mean suffers in dependability from its sensitiveness to the most discordant values in a set of observations, the inclusion or rejection of these values having more effect on the a.m. than in the case of any other values. On the other hand, the median (i.e., the middle value of all values in a set ranged in size order) is equally affected by the inclusion or rejection of any of the readings.

In practice the "huge error"—or to be more accurate the huge residual—is the bugbear of all sets containing only a moderate number of readings, and this argument points to a real advantage possessed by the median over the a.m.

I am not advocating the use of the median in place of the a.m., but I do assert that, in the absence of any really well founded argument, the evidence does not all point one way, and the final choice of a suitable function of a set of observations, for the purpose of evaluating the measured quantity, must remain largely a matter of individual taste. What sacrifice of accuracy balances a given gain in security, and what is the measure of the latter in any given case? Perhaps the chief defect of the median is its resistance to further mathematical manipulation; this is not always a disadvantage.

etc.) do represent something, especially when comparing very similar sets of observations; but these applications of the law must not be pressed very far. What is meant by "very far" must depend on the particular circumstances of the case. My experience (which will be different for every one) is that it rarely pays to repeat an observation more than 10 or 15 times; or, at any rate, however often repeated, the precision of the a.m. should not be taken to be more than about 3 times as good as that of a single observation. Thus, if the m.e. of each observation of a set were ϵ, the least m.e. that could be obtained for the a.m. by mere repetition is about $\frac{\epsilon}{3}$, and that from 10 to 15 repetitions are usually sufficient.

There is one question which often faces the designer and observer, namely, what is the greatest allowable m.e. of an observation in order that its maximum error may never exceed some error ϵ? Absolute certainty does not belong to this world, and, in practice, the question should be recast by making some assumption as to how unlikely the event is to be. This is exactly where the Law of Errors is most unreliable, and the answer partly depends on the class of instrument and the circumstances of its use. No numerical data are of much use and experience is the only safe guide. My experience is that the error which, by the accepted law, has a chance of 1 in 100 may be expected continually, and that the 1 in 1,000 "huge error" is by no means uncommon. For anything like a great improbability on any one observation the nominal chance should be something between 1 in 10,000 to 100,000; but, as said above, this almost entirely depends on circumstances.

It is perhaps surprising, in view of the above discussion, that the Law of Errors fits practice so well as it does. This may be due to the fact that the resultant disturbance, surrounding an observation, enters that observation through a number of elements, and the idiosyncrasies of the latter tend to cut each other out; but as already observed, the number of elements which are likely to be noticeably disturbed are usually not many, and one or two may well overshadow the rest, so too much cannot be expected.

CHAPTER V

ERRATIC ERRORS (INCOMPLETE ELEMENTS)

GENERAL

An incomplete element has already been defined as an element such that the relation between the two links, indicated by the restoring agencies, contains an unwanted degree of freedom. This does not mean that one link is free to vary without limit in this particular sense without encountering a restoring agent, but that some finite movement is unopposed. Thus, a spindle rotating in an oversized journal represents an incomplete element.

Such elements commonly, but not always, lead to erratic errors, as will be shown; but, by adding some constraint which functions as a restoring agent, this error can often be eliminated; sometimes this constraint already exists, and, by a proper manipulation of the instrument, can be kept constant, at least in sense. This, of course, completes the element.

THE LAST ELEMENT

There is one element in practically every observation which is incomplete, namely, that consisting of the indicated reading and the mind of the observer estimating this reading. This is the last element in an observation. A measured quantity, such, e.g., as a length, might conceivably be expressed as 15.427358269 . . . mm., but in practice it never is, and for the very good reason that no method of estimating beyond a given number of significant figures is possible with any particular instrument. If a simple scale is used the answer will probably be expressed as 15.4 mm., the least count being 0.1 mm. On the other hand, with a microscope the result might be expressed as 15.427, the least count being 0.001 mm. In one important sense the last example is not a closer approach to a graduated reading than the former, for in both cases the least count is of the same order of magnitude as the m.e. of the observation.

In the case of null instruments involving measurement, the setting itself is graduated provided that the standard quantity (or its relation to the unknown) can be altered by infinitesimal steps, as in the case of a sliding resistance Wheatstone bridge; but even here a least count is involved in determining the value of the standard quantity. Only when the operation consists in equating two quantities without making any attempt to measure either are errors of integers avoided, and then only in certain cases.

The relation between the m.e. of a single observation performed with an instrument and the least count of that instrument is in the hands of the designer, for it is usually not difficult to increase the sensitivity, or, what amounts to the same thing, decrease the least count almost indefinitely. This "gearing up" of the pointer or indicator does not increase the inherent accuracy of the instrument, but it wipes out any significant error due to the last element.

In practice a sensitivity disproportionately great in comparison to the accuracy of an instrument is objectionable for several reasons. In the first place it is misleading, for the accuracy of any instrument is commonly supposed to be roughly indicated by its sensitivity. Then it is irritating to have to record a number of additional figures which are not significant. Finally an instrument with "empty" sensitivity inevitably gives an impression of unreliability. On the other hand, too low a sensitivity leads to unnecessary error and one which cannot always be wiped out by repeated readings, for, unless the scatter of the actual indicator positions, for a set of similar observations, spreads over several times the least count of the scale, the errors due to the latter will not even out. In practice the least count should be of the order of 5 to 10 times less than the m.e. of a single observation, and should always be less than the m.e. of the a.m. of sets. Usually the indicator will move over a scale graduated to the penultimate significant figure, and in general it is good practice to arrange that the graduations should have about the same value as the m.e. of a single observation, and be read by estimation to tenths; this satisfies the rule just given and also gives the observer a sense of security with the instrument in that it is not graduated beyond its accuracy.

Observers vary greatly in their powers of subdividing graduations by estimation, but they should not be expected to subdivide any interval into more than 10 parts; an easy rule to remember is that one-tenth inch held at 10 inches from the eye is about the smallest interval which can be comfortably divided into tenths under favorable conditions as regards scale markings, pointer illumination, and eyesight. If a vernier is provided, this replaces the estimation, but does not increase the sensitivity though it may minimize errors of judgment, and by its help sometimes enables the least count to be halved; thus, under favorable conditions, an interval of one-tenth inch at a distance of 10 inches can be subdivided into 20 parts with a suitable vernier.

A difficulty arises, for most instruments have several accuracies depending on the method of measurement adopted, and the highest possible accuracy must be catered for; also neither the accuracy nor the sensitivity are always independent of the quantity measured; here common sense will usually enable a reasonable balance to be struck between least count and m.e.

EXAMPLES

A few typical cases of incomplete elements will now be considered. Consider a nut engaging its leading screw. As already explained, the microscope of a Hilger measuring instrument is fast to a saddle on a slide, the former being moved by the motions of such a nut. If the nut has any end shake on the screw, equal say to $x\ \mu$, the relationship between the position of the nut and the rotation of the screw is indeterminate within the limit of $x\ \mu$. Hence, other errors being assumed absent, any measured length L may be in error by this amount; for,

$$L = l_2 - l_1;$$

and the possible extreme settings are

$$\begin{array}{lll}
l_2 + x - (l_1 + x) & = l_2 - l_1; & \epsilon = 0. \\
l_2 + x - (l_1) & = l_2 - l_1 + x; & \epsilon = +x. \\
l_2 - (l_1 + x) & = l_2 - l_1 - x; & \epsilon = -x. \\
l_2 - l_1 & = l_2 - l_1; & \epsilon = 0.
\end{array}$$

It has here been assumed that the correct reading is obtained when the whole backlash is taken up in one direction; since this instrument has no real zero, and only measures by difference, any one position of the nut relatively to the screw can be defined as correct, other possible positions then having errors deduced for them.

Provided that the whole backlash is taken up one way or the other,

$$\epsilon = \pm x, \text{ or } 0;$$

and provided the backlash is taken up the same way on each of the two settings l_1 and l_2, then,

$$\epsilon = 0.$$

It is, of course, the invariable practice to come up to all readings in the same direction for this very reason, but even so some doubt must exist as to how far the whole of x has been taken up. To put it another way, is x a very definite quantity? Backlash may be defined as the maximum variation of an element which is unattended by any restoring agency, but something more than backlash is involved here. In the present case it is the relationship between two parts which is capable of so varying. If (Fig. 20) the surfaces of the threads A and a, B and b, were mathematically flat, parallel and clean, the positions at which the restoring agency cut in could be precisely stated, but none of these conditions really hold. The surfaces are covered with oily films which, undoubtedly, are capable of withstanding a considerable compressive stress when thin. These must be responsible for some restoring moment, rising rapidly as the films are compressed. The films will not be of uniform thickness initially, whilst, under compression, this thickness from place to place will depend on the imperfections of the metal surfaces. When the faces Aa, say, are sufficiently close to be oil sealed they will continue to approach until the restoring moment equals that due to the friction between the microscope's saddle and slide. Since this friction is itself liable to variations, and the thickness of the oil films on the threads varies every time the screw is turned, this position of balance is uncertain—far more uncertain than when Aa and Bb are simultaneously under compression.

Figure 24 illustrates the point diagrammatically.

In (A) the metal surfaces of the nut A and B are shown as vertical lines separated by the distance OO_1, whilst the surfaces of the screw a and b are similarly shown separated by the distance PQ. The four surfaces are supposed to be covered with films of oil, not necessarily of the same thickness. The curve HGF represents the pressures, sustained by the combined films on A and a, on a base representing the separation of A and a; thus, if surface a cuts the curve at G, the separation of A and a being OG_x, then the pressure of the intervening oil would be OG_y. When a is separated from A by OF or more, the pressure is zero. If this pressure is taken to be the total force exerted on A and a by the oil, then OG_y becomes the restoring moment when a cuts the curve at G.

Beyond F the films on the two surfaces are not in contact. The curve HGF is purely diagrammatic, but correctly illustrates the fact that, as the distance between A and a decreases, the pressure rises slowly at first, and finally increases very rapidly when the film is only a few molecules thick, most of the subsequent strain being in the metal threads themselves.

The curve MLK represents precisely the same set of facts for the surfaces b and B, L_y being the total oil pressure on B and b, when the latter cuts the curve at L. The curves HGF and MLK are not in general identical in form owing to variations in film thickness, and the screw thread, ab, can take any position with respect to the nut AB within the range of values $(FK-PQ)$ before any restoring moment is encountered. If the friction between saddle and slide forces a towards A, ab will move relatively to the left until P occupies some position G_x such that

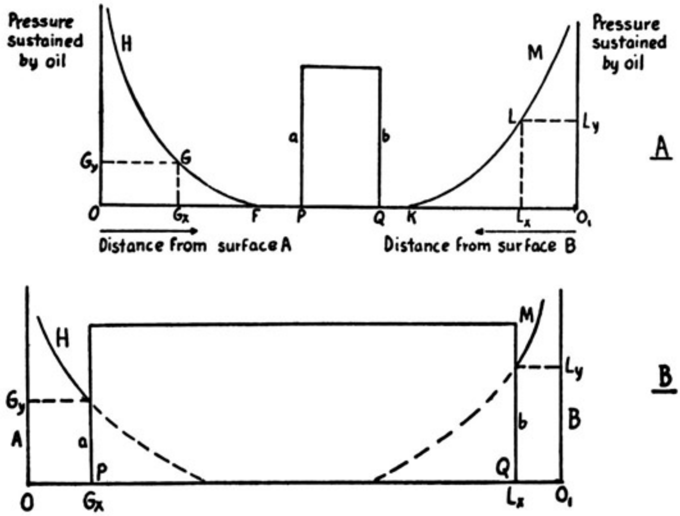

FIG. 24.

$$G_y = \text{friction force.}$$

The stability of this relative position of the two surfaces depends on the constancy of the saddle friction and G_y as the screw is rotated. If G is close to F the friction is relatively small, and G_y is correspondingly light and liable to fluctuations, depending on the oil swept up as the nut advances. Moreover, any change in the friction force or G_y results in a comparatively large movement of P along OO_1 before the new balance is struck, owing to the small slope of the tangent to the curve at that point. Conversely, if the saddle friction is great G, the intersection of a with the curve, will move towards H where the curve is rising steeply, and fluctuations of G_x will be very slight.

This illustrates the well known principle that, if lost motion exists in any instrument, sufficient force should be added to make sure that it is taken up. The addition of unwanted forces is, however, a heroic remedy accompanied by disadvantages of its own, and (B) represents the best solution. Here the screw thread ab more or less fills the nut and the faces a and b both cut their respective curves where the latter are rising steeply; if the saddle friction is forcing a on to A then,

$$G_y - L_y = \text{friction force};$$

and since the curves are steep at G and L (much steeper relatively than the diagram suggests) G_x and L_x are nearly independent of the friction force, the strain being largely taken by the metal threads. There is a practical limit to G_y and L_y, for, if the films are ruptured and A_1a or B_1b make metallic contact, these surfaces will be damaged, and, short of that, μ is not negligible when a and b are sliding over A and B (even when the films are intact), and undue resistance to motion will strain the screw and other parts resulting in erroneous readings and making delicate settings difficult; hence, it is important in this case to keep the saddle friction as light as possible.

This example illustrates the uncertainty of the relationship between two parts with backlash unless extraneous forces are added sufficient to force the two parts into one of their extreme alternative relations. When this is done with a minimum of force the result is apt to be large positive and negative deviations, but worse still these deviations are not unbiased. For in (*A*) a given excess in the saddle friction will not force G_x so far to the left as a similar deficit will cause it to travel right, since the curve is steeper on one side of G than the other; this is an almost universal characteristic of "unbalanced" restoring agencies. (*B*) represents a "balanced" system of restoring agencies which, if not quite symmetrical, will be nearly so.

In general it is evidently unsafe to establish a mean position by providing very different sorts of restoring agencies for "overs" and "unders" (e.g., oil pressure and friction) unless the consequent control is so good that significant deviations are not expected, as the laws connecting restoring moments with deviations will be very different on the two sides and no normal frequency law can be expected to apply. In the case just considered, whenever the instrument is in use, the nut is being moved along the screw, and it will naturally happen that *Aa* or *Bb* are being forced more or less successfully together; *ab* is never left without some sort of guidance; but this is not a necessary feature of an incomplete element.

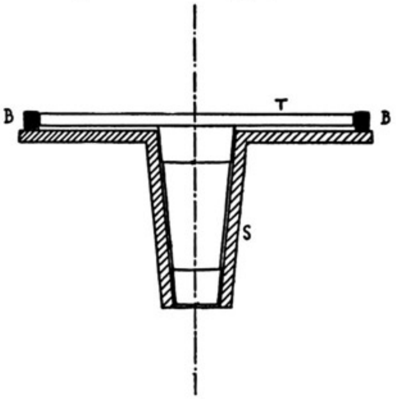

FIG. 25.

Figure 25 diagrammatically represents a very general type of friction control. The table *T* is rotatable with respect to its base *S*, and is prevented from unwanted rotations by the friction pads *B*, which in this case are spring loaded. In practice *B* may be friction clamps, added friction constantly operating, or merely the natural friction of any moving part which normally prevents unwanted movements. Provided the friction force is great enough to prevent slip between the surfaces of *B* and *T*, the required relation between *T* and *S* (i.e., the maintenance of some relative angle) is cared for by a restoring agency, which consists of the resistance of the various parts to deformation. If, however, *T* slips with respect to *B*, there is no restoring agent endeavoring to restore the original relation of *T* to *S*. The resultant error is erratic in that its value is incalculable (assuming the change of angle to be unknown), the constancy of the error is a matter of chance, and no number of observations however large will ever approximate to the value which would have been found had *T* remained fast to *S*. The result of repeated observations makes the chances of error worse rather than better.

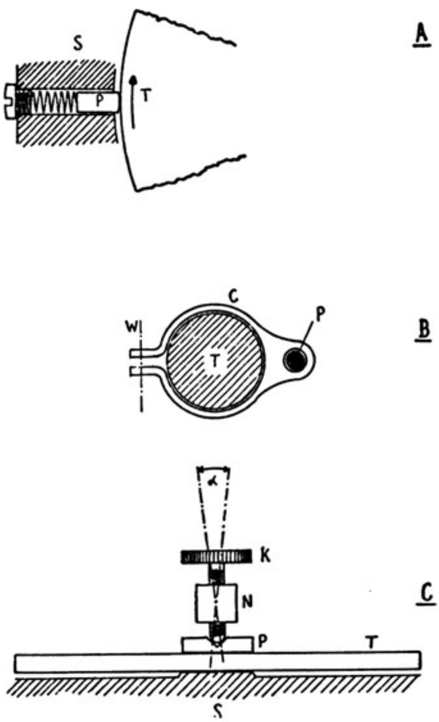

FIG. 26.

A somewhat modified form of this is not uncommon in the types of clamps shown in Fig. 26. (*A*) represents a spring loaded plunger *P* controlling the movement of the rotating (or sliding) member *T* relatively to the stand (or other part) *S*. Assuming that no slip occurs between *P* and *T*, some movement is still allowed if *P* is a loose fit in *S*. Here the movements of *T* are limited by the possible play of *P* in *S*, and if a very large number of observations were taken they would perhaps approximate to that value when *P* was central in its hole. This is poor comfort for several reasons; the original setting will have been made with *P* at one side or other, thus if *T* travelled in the direction of the arrow *P* will be on the upper side of the hole. Apart from this, the mean position of *P* is merely a statistical mean, no restoring agency is acting, and all possible positions of *P* are equally likely a priori; consequently the precision of the observations will be low, and an incredible number of observations would be required. Actually some bias would render the task even theoretically hopeless.

The great advantage of a restoring agency is that it enormously adds to the probability of small deviations as compared to large ones by making the latter definitely unstable when the disturbing cause is removed.

(*B*) shows a clamp liable to the same sort of defect. The split collar *C*, anchored by the pin *P*, clamps the shaft *T* when the wing nut *W* is tightened; any slackness of *P* in the hole provided in *C* results in the condition just discussed.

In (*C*) the knurled thumb screw *K* clamps the table *T* between the slipper *P*, and *S*. If *K* is slack in *N* it will be capable of wobble; this may not be serious if the friction between *T* and *S* is sufficient to hold the table, but if the slipper has not only to create a pressure between *T* and *S* but also to hold *T*, the same arguments apply as for cases (*A*) and (*B*).

Case (*B*) in particular is liable to cause an error independently of those considered. Apart from any slackness of fit at *P*, the two arms of the collar rarely close on to *T* quite symmetrically as *W* is tightened, and, consequently, *T* will be very slightly rotated out of its correct setting in the act of clamping. This deviation will be biased, usually acting in one direction for a given instrument, but will not be equally large on the various occasions; the resultant error is erratic.

Figure 27 shows a slide *S*, controlled by the adjustable and fixed gibs *A* and *B*, and sliding as well on the surfaces *C* and *D*. When used in small sizes, both gibs are usually fixed, and the fit attained is entirely a matter of workmanship. Unless the latter is of a very high quality, either *S* will be capable of a certain amount of side play, like a badly fitted chest of drawers, or the slide will be stiff to actuate. If the sliding surfaces are slightly convex to each other, both conditions may easily obtain simultaneously.

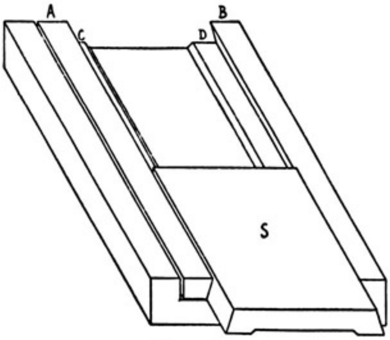

FIG. 27.

The effect of play has, in effect, already been considered but the friction forces are apt to result in erratic errors of an aggravated kind with this device. For not only is the friction often great, but unless the workmanship is excellent the friction will vary widely from place to place; for instance, a very small degree of "wedginess" in the gibs will result in the slide being slack at one end or unduly tight at the other, or both. Again the difference between stiction and friction is usually very marked. Now assume that such a slide is actuated through a train of links, involving perhaps a torque transmitted through a relatively long shaft. Then, unless the intermediate mechanism is very stiff, the position of the slide relatively to the position of the first member of the train will be subject to considerable uncertainty through the strain in the intermediate links, due to the very variable friction forces associated with *S*. On occasion the difference in the values of stiction and friction can result in a state of affairs closely analogous to the oscillatory discharge of an electric accumulator. Thus consider S stationary, then assume the first link, in an elastic train controlling S, to start moving uniformly. At first S will not move, the train being strained; then, when the stress rises sufficiently, the stiction is overcome and S moves, with the result that its resistance to motion (due now to friction) falls; if S is not sufficiently damped by friction, it will overshoot and again come to rest; the cycle will then be repeated. Every engineer will recognize this as a case of "chatter", often met with in machine tools if too heavy a cut is taken, and by no means unknown in instruments in a milder form.

Friction and Erratic Errors

Most frequently the relation between two links in an element is spatial, and, in the presence of friction, many otherwise complete elements with spatial relations become effectively incompete.

Let the full line in Fig. 28 represent the relation between the moment of the restoring agencies M_a in some element and its disturbance. The essential feature of this moment, which in general is not symmetrical about its zero, is that it increases smoothly from zero with the disturbance of the element. The relative movements, i.e., the disturbance, between two links may be attended with friction which is yet insufficient to prevent that disturbance. Then, since friction opposes movement in both senses, the friction moment M_f, supposed constant, can be represented by the dotted line parallel to the X-axis. M_f cuts M_a in two places P and Q corresponding to the two senses of disturbance.

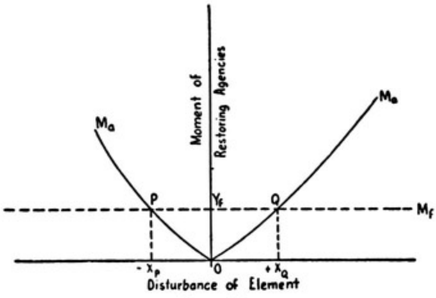

FIG. 28.

Let P correspond to a disturbance of $-X_p$, and Q to X_q, then, if the element be disturbed to any extent between $-X_p$ and $+X_q$, the moment M_a, being less than M_f, will be insufficient to restore the element. Thus, the element behaves as though it were incomplete between the limits $-X_p$ and $+X_q$; for the element will stay put with its relation anywhere in between these two extremes.

This range of uncertainty can, of course, be reduced by reducing the friction, but the latter can never be quite eliminated in moving parts, and it is only with suitable design that a reduction in friction does not reduce the moment of the restoring agencies in another direction. This is discussed in Part II. To eliminate this type of uncertainty in the presence of friction, M_a must maintain some finite value, greater than M_f, up to the moment when the disturbance is zero, when M_a must abruptly fall to zero too.

This condition is approximately met by the mechanical detail shown in Fig. 29, which represents an end view of a steel bar C, free to slide on the guides A and B, and loaded by the spring S. The bar is supposed to be similarly constrained at its far end, and the object of the guides is to provide a restoring moment which will prevent lateral play. Ignoring any change in the force exerted by the spring, any disturbance right or left is met by a restoring moment F Cot θ, where F is the force due to the spring and the weight of C. This is sensibly constant whatever the amount of the disturbance, since, if the latter be to the right, the restoring agency for left disturbance, i.e., guide A, is out of action; and

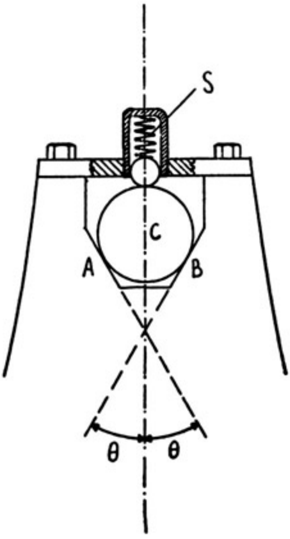

FIG. 29.

vice versa. Figure 30A shows the relation of restoring moment to disturbance. The former is shown constant except for the zero position, where the agencies for disturbances right and left are both in play. Hence, so long as any friction moments, tending to hold the bar over to one side, are less than F Cot θ, the element will be undisturbed. This ignores the deformation of the bar and guides, which is supposed to be below the significant level; the true M_a, D diagram is shown in Fig. 30B.

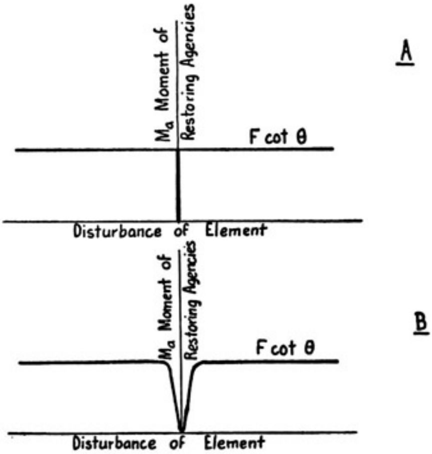
M_a Moment of Restoring Agencies
A
F cot θ
Disturbance of Element
M_a Moment of Restoring Agencies
B
F cot θ
Disturbance of Element

FIG. 30.

Many elements are incomplete in themselves and rendered more so by reason of friction; this is shown in Fig. 31 where in the absence of friction the relation between the two links is indeterminate between the range $-X_1$ to X_2, whilst in the presence of friction this range is increased to $-X_3$ to X_4. It will be noticed that, friction apart, the element sketched in Fig. 29 is *inherently* incapable of being incomplete, whatever the workmanship and irrespective of wide fluctuations in the controlling force.

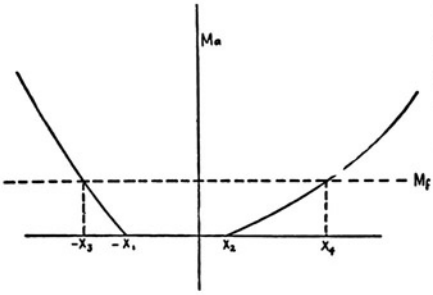

FIG. 31.

If a train of mechanism consists of one or more incomplete elements, it will possess backlash; but, as has already been seen, this is not necessarily serious provided that the measurements required are differential, and the backlash is capable of being taken up in the same direction on each observation. Ordinarily, complete elements rendered incomplete by friction are dangerous, for the latter is notoriously subject to irregular change, and the "lost motion" will vary from one observation to the next.

An instrument whose elements display the symptoms of incompleteness will show *hysteresis*,[1] in very much the same way as a piece of metal under test. If such an instrument be used to take readings on a series of quantities in ascending order of magnitude, and then the readings be repeated in the reverse direction, two curves connecting reading and quantity are obtained, one of which will lie wholly below the other; the distance between the curves at any point gives the value of the backlash involved in measuring that particular quantity.

MENTAL RESTORING AGENCIES

Incomplete elements are not necessarily mechanical, for the mind seems to be capable of supplying restoring agencies which are liable to indicate different relations on different occasions (this is analogous to the mechanism shown in Fig. 25), and also agencies which indicate nothing within certain limits.

An example of the first is when a fine graticule line has to be set on a coarse object; if an observer takes a set of observations and repeats the set after an interval, the a.m. of the two sets is often separated by an interval quite out of proportion to the precision of either set; the presumption is that a new relation is being indicated on the second occasion. A very skilled observer can make a first-class

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[1] An excellent discussion of instrumental hysteresis is given by F. J. Schlink in the Bureau of Standards Scientific Paper, No. 328, and a shorter account in Professor Pollard's little book *The Kinematical Design of Couplings in Instrument Mechanisms;* published by Messrs. Adam Hilger, Ltd., London.

"cut" of the range finder type with a m.e. of perhaps 3 to 5 seconds at the eye, though he cannot appreciate an error to anything like this precision if presented with a "cut" as a fait accomplis. It is difficult to imagine that, if presented with a "cut" containing an error of say 1 second at the eye, anything occurs in the observer's mind which could be described as an indication (however faint) as to the sense or even the existence of the error.[1]

An example of mental restoring agencies will be given in the next chapter.

There is one important respect in which mental and material restoring agencies are quite unlike; namely, in the moment they exert to restore a disturbed element. The realization of the error may be more or less vivid depending on the size of the latter, and so the one is, in general, a function of the other. There will also be more or less certainty attached to the sensation, but, assuming that an opinion is formed as to the sense of an error, the means for restoring the element (i.e., the hand employed to actuate some control) is never inadequate to produce a change. Hence, in the case of elements with mental restoring agencies, friction will not cause it to become incomplete; unless indeed the means of control are so burdened with friction that a fine adjustment is impractical; but this amounts to a mechanical blunder.

CONCLUSION

Erratic errors have this in common with short period errors, that they are inconstant; but they do not fluctuate about a settled mean, nor are they a function of the resultant disturbance surrounding an observation at any given instant. When the disturbance ceases, short period errors vanish, but this is not true of an erratic error, which often persists indefinitely until a new disturbance changes its value. Consequently, successive disturbances result in an erratic error which is often the resultant of the successive errors associated with each disturbance. An instrument has a memory for erratic errors.

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[1] See *The Basis of Sensation* by Professor E. D. Adrian for a description of the "all or none" character of nervous reaction.

The two most fruitful sources of erratic errors are elements which are incomplete, and otherwise complete elements in the presence of friction. Incomplete elements result from inferior workmanship, together with a design which only permits an element to be complete in proportion to the excellence of the workmanship (e.g., Fig. 27), as distinct from an element whose completion is independent of dimension (e.g., Fig. 29).

In the case of a machine the friction forces are largely due to the weight of the parts or the magnitude of the forces to be transmitted; in most instruments this is not so, for the parts are usually light and no work is involved other than that needed to overcome friction. Most of the friction in an instrument can be traced to a desire to avoid incomplete elements, where these are not complete by design as distinct from manufacture. The former will be called *inherently complete elements*, and have the great advantage that as they do not require large forces and therefore great friction, so they do not necessitate that very quality which is the chief cause of incompleted elements and of hysteresis. Moreover, comparing the mechanisms in Figs. 27 and 29, the friction will only vary with μ (the coefficient of friction) in the latter case; whilst, in the former, the forces themselves are subject to fluctuation as the slide moves along its guides, causing the friction force to be dependent on a second variable.

CHAPTER VI

CONCLUSION OF PART I, AND AN EXAMPLE

Summary of Part I

The various kinds of errors have been examined separately, and it has been shown that, as a rule, each error—systematic, short period, and erratic—may be traced to a particular type of faulty element.

Two classes of errors are amenable to correction by method and calculation, systematic errors and unbiased errors; only the first of these corresponds closely to a natural group; the second, which really implies that the errors also follow the accepted "Law of Errors", is largely a creation of mathematicians, but use can be made of this law provided only that short period errors are unbiased.

The central problem in design is first to try to eliminate errors; secondly, to persuade such as remain to figure in measurements as systematic or unbiased errors; and thirdly, to provide the necessary facilities to enable the observer to grapple successfully with these two classes.

Obviously the most hopeless error to meet in precise measurement is the erratic error. It cannot be calculated and methods of measurement can do nothing to help, *so long as the error remains erratic*, because these methods practically always assume that the error will remain constant for a duration of time sufficient to complete all the observations composing the measurement.

Fortunately, as will be shown in Part II, erratic errors are the easiest to eliminate by design. The conditions for complete elements are: (1) The reduction of friction to its lowest possible value. (2) Restoring agencies whose moments never fall below some definite finite value when any significant disturbance of their elements occurs, however small. (3) Elements which are inherently complete; that is, complete by virtue of design rather than execution. These three conditions are simultaneously met in many cases by what is called geometric, or kinematic, design.

Again, in the case of elements which are incomplete between narrow limits, it is sometimes possible to make the error systematic by driving the relation to one of its two extreme forms and holding it there—e.g., always taking up backlash in one direction. This is really equivalent to completing the element by adding a more or less constant restoring agency. The systematic error sometimes takes the form of a zero error, which can often be eliminated by taking a differential measurement, or a measurement by substitution. By satisfying condition 2 above, short period errors are also greatly reduced; but, though erratic errors can often be depressed below a significant level, some significant short period and systematic errors always remain if the highest possible accuracy is being sought.

It has already been seen that if the design is departed from in manufacture, systematic errors will occur; though the most perfect workmanship imaginable will not necessarily prevent these errors if the design is not adequate. In any case, an instrument cannot be more free from these errors than the quality of manufacture, and facilities for adjustment, permit. Method in measurement and suitable calculation will do something to remove the remainder; but this has its limits, and finally the residual error, if significant, can be made short period if only facilities are provided for breaking down and readjusting the faulty element between repeated measurements. This is likely to be a tedious operation and is only suitable on some occasions; the first care of the designer should obviously be to so proportion the parts as to make the various elements as insensitive to unavoidable errors of manufacture and wear as possible. Some examples will be given later.

Short period errors are strictly only capable of treatment if they obey the accepted law of the frequency of errors; but, as has been seen, they can be reduced to a fraction of their value if only they are reasonably unbiased. Their lack of bias has largely to be taken on trust, and this is one of the reasons why it is useless to repeat a measurement a large number of times; it has been suggested that 10 to 15 repetitions, leading to a reduction of the m.e. to about one-third, is a usual practical limit. At the same time the designer can often avoid obvious asymmetry which would palpably lead to biased short period errors.

ELEMENTS WITH MENTAL RESTORING AGENCIES; AN EXAMPLE

The following is an account of some measurements taken a few years ago which are an admirable example of mental restoring agencies, and illustrate all three types of errors. This example was not chosen as being unusual, given the type of elements involved; in fact it is quite typical, and is literally the first record of measurements I found when looking over old papers.

The object of these measurements was to determine the accuracy with which a particular telescope could be set on a collimated line. Moreover the conditions of the problem were peculiar and depended on the nature of the instrument into which the telescope and collimator were to be incorporated.

The telescope and collimator were identical in that each had similar object glasses, plano-convex lens of about 6.6 inches focus, and having a diameter of only about 0.25 inch. At the focus of each lens was a graticule, that of the collimator being etched with a single line, whilst the telescope graticule had two parallel lines. The telescope carried an eyepiece with a focal length of 0.4 inch. To perform a setting, the telescope was angled in azimuth until the collimator line fell symmetrically between the two lines on the graticule of the telescope. As some doubt existed as to the accuracy which could be attained with these very inferior objectives, the following experiment was performed.

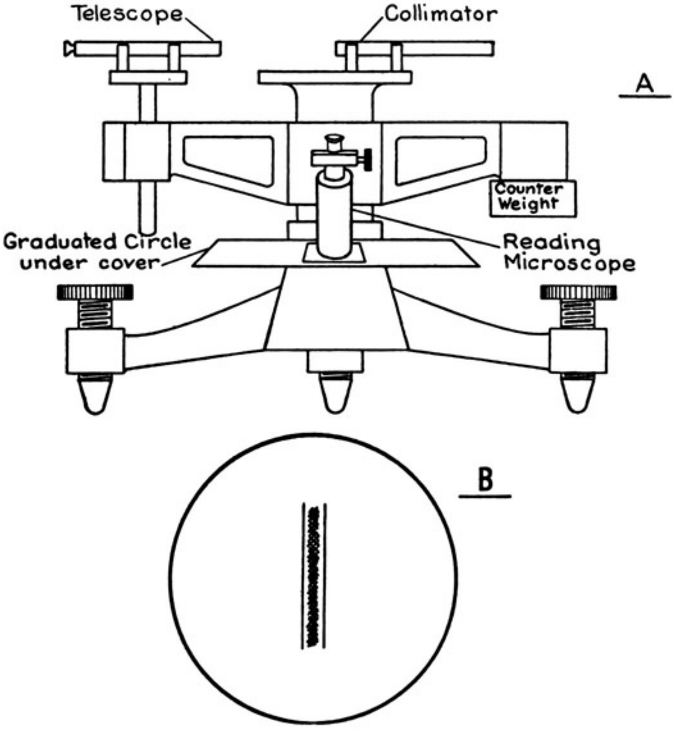

FIG. 32.

Figure 32(*A*) shows the general lay-out. The collimator was fixed, and the telescope placed on a goniometer which read by estimation to 0.1 second of angle. The method adopted was to bring the collimated line into register between the two telescope lines, and then to take two readings on one of the reading microscopes; the mean of these two readings being adopted as the value of that setting. Ten settings of the telescope constituted a set of readings. The observed scatter of the readings included the scatter of the telescope setting as one factor; other possible causes of error were:

a. That the telescope or collimator had moved in their mounts, or that the latter had suffered relative movements apart from the intended rotation.
b. Lack of precision in reading the reading microscope.
c. Systematic errors in the goniometer, including microscope.
d. Erratic errors in the goniometer, including microscope.

With regard to (a) precautions were taken to avoid this; the apparatus was very compact, and these movements must have been exceedingly small compared to other causes of error.

(b) Accounted for errors of the same order (but less than) the effect to be measured; sufficient internal evidence exists to evaluate these roughly, as will be shown later.

(c) The circle of the goniometer was only used to give one reference line, for the whole scatter of the observations was well within the range of the micrometer eyepiece of the reading microscope; consequently no questions of centring error, or irregular divisions of the circle, arose. The micrometer eyepiece was not tested for systematic error, but this is not likely to be serious as only a moderate per cent accuracy on its travel was called for. It must be remembered too that the effect of the microscope's objective is to gear up the movements of the goniometer, and, to that extent, reduce the demands on the micrometer eyepiece.

(d) The chief danger here is due to lack of a definite restoring agency in the micrometer slide, but several hundreds of readings were taken and no gross "jumps" were noticed such as usually accompanies marked lack of control. The micrometer readings were all taken by bringing up the slide in one direction and nearly all micrometer readings were taken by one observer. The only other real possibility is uncertainty of movement at the pivot of the goniometer. This was well made, had not had very much use, and the movement was always exceedingly small.

It is fairly certain therefore that the scatter of observations was almost entirely due to uncertainty in setting the collimated line between the two telescope lines, and uncertainty in making the same type of cut in obtaining the microscope readings.

The latter cause of scatter would have been unfortunate had the actual accuracy of the telescope-collimator setting been required; all that was really wanted was to ensure that it did not exceed a limit far below the accuracy of the goniometer. A setting as it appeared through the eyepiece of the telescope is shown in Fig. 32(*B*). The coarse central line (from the collimator) was, in fact, as finely engraved as the other two, and viewed with the same nominal magnification, but appeared fuzzy and poorly defined owing to the nature of the optical system. Since the scatter of observations in this case is due to two visual settings of the same nature, they will be lumped together in the first instance.

Table II gives five sets of observations, each set containing ten settings of the telescope, and determined usually by two microscope readings. Since the latter were not the object of investigation, if the two readings were unusually discordant, a third reading was taken and meaned up with the other two; this occurred twice in this table, and consequently huge errors are unlikely in the case of the microscope readings. Each set was taken by a different observer, *A* being by far the most experienced and reliable (in the sense of having a definite equation); *E* had little or no experience of accurate measurement, whilst *B*, *C*, and *D*, were intermediate. It is well to remember that a high precision on the part of an inexperienced observer when judging a fuzzy cut (as in the case of *D*) is not necessarily a sign of grace. It is apt to indicate that the observer only used his judgment once, and then relied on visual memory to repeat the first setting. The test is to give the observer a good lunch, and then see whether his equally precise afternoon observations bear any relation to the previous set.

In the table the residuals are obtained by subtracting the mean for the set from successive observations; whilst the m.e. is, of course, the mean residual obtained by summing the latter without regard to sign, and dividing by the number of observations in the set.

TABLE II

Observer	*A*		*B*		*C*		*D*		*E*	
	Reading in Secs.	Residual	Reading in Secs.	Residual	Reading in Secs.	Residual	Reading in Secs.	Residual	Reading in Secs.	Residual
	6.2	+0.4	9.7	+1.6	3.9	−1.5	5.5	−1.5	7.9	+2.7
	5.2	−0.6	8.2	+0.1	5.7	+0.3	6.8	−0.2	7.7	+2.5
	4.9	−0.9	12.0	+3.9	4.5	−0.9	5.9	−1.1	3.8	−1.4
	5.8	0	8.0	−0.1	6.1	+0.7	7.1	+0.1	6.7	+1.5
	7.0	+1.2	7.8	−0.3	3.1	−2.3	7.7	+0.7	1.6	−3.6
	7.9	+2.1	6.1	−2.0	5.7	+0.3	6.0	−1.0	2.3	−2.9
	5.9	+0.1	8.5	+0.3	5.2	−0.2	7.3	+0.3	7.5	+2.3
	4.8	−1.0	7.9	−0.2	7.5	+2.1	7.7	+0.7	3.2	−2.0
	4.9	−0.9	7.8	−0.3	5.3	−0.1	7.4	+0.4	5.8	+0.6
	5.4	−0.4	4.7	−3.4	6.6	+1.2	8.2	+1.2	5.4	+0.2
Means	5.80		8.07		5.36		6.96		5.19	
Mean error	0.76		1.22		0.96		0.72		1.97	

The image of the collimated line was brought into setting from the observers' left.

The means and m.e. are given to 0.01 second of angle; the last figure has little or no significance, and should finally be rounded off, the object of retaining it is that the arithmetic cannot be checked conveniently without its help. Moreover, it is a sound principle to round figures after all arithmetic has been performed; intermediate approximations are apt to grow surprisingly.

Assuming, for the moment, that the residuals of each set have a frequency law not unlike the accepted law, the mean error of a set is equal to,

$$\frac{\text{m.e. single observation}}{\sqrt{\text{Number of observations in the set}}}.$$

Hence, the various m.e. of the sets, in this case, are about one-third those of the observations forming the set. Thus (rounding the last figure);

Observer	*M.E. Set, Seconds of Angle*	*Arithmetic Mean of Set from Table II*
A	0.25	5.80
B	0.40	8.05
C	0.30	5.35
D	0.25	6.95
E	0.65	5.20

In Fig. 33 this is illustrated graphically. Parallel to a scale representing seconds of angle, a line has been drawn for each observer. The length of the line is equal to twice the mean error of that observer's set (i.e., + and − the m.e.); and the midpoint of the line is opposite the scale reading corresponding to the mean reading of the set.

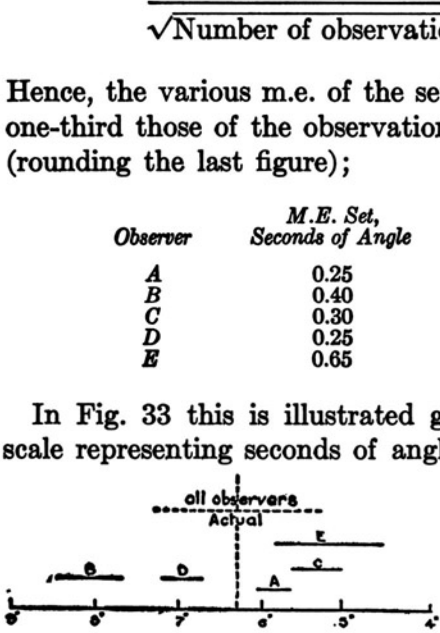

Fig. 33.

The dotted vertical line represents the mean of the various a.m. of the five observers, and, curiously enough, it happens that this mean of means has sensibly the same value whether each observer be given the same weight, or be weighted inversely as the size of their respective m.e. (6.27 and 6.3, respectively).

The whole figure is most unconvincing; it strongly suggests the presence of errors other than those represented by the various m.e., and this can be immediately tested as follows:

Observer	*Arithmetic Mean of Set*	*Residual*
A	5.80	−0.50
B	8.05	+1.75
C	5.35	−0.95
D	6.95	+0.65
E	5.20	−1.10
Mean of means	6.3	

M.E. of mean of means = 1.0

This M.E. is greater than the individual m.e. of any of the observers; it is shown on the top of the vertical dotted line in Fig. 33. Common sense indicates that, almost certainly, the unbiased errors, causing the scatter in each observer's set, cannot be the only errors at work. Assuming these errors to be due to the human element, the question arises as to what human defect, or limitation, is responsible for these divergencies. It may be pointed out that a physical defect of the eye, regarded as an optical instrument, could hardly account for a bias in this instance; consequently, the cause would seem to be psychological. Table III throws some more light on this question. The readings so far analyzed were all taken by causing the collimated line to

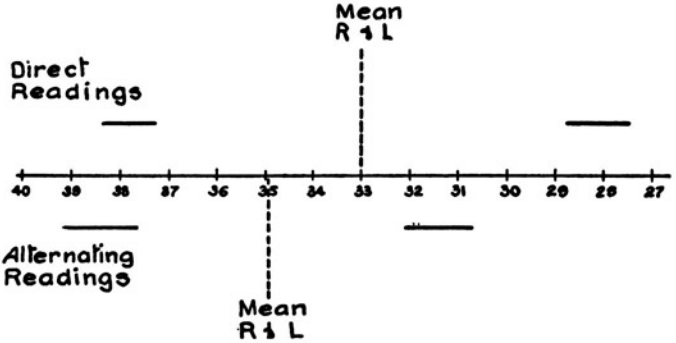

FIG. 34.

travel from left to right in the field of view. Subsequently, some of the observers took sets with the line travelling from (a) left, (b) right, (c) left and right alternatively. In Table III results obtained by observer *A* are recorded; the other observers all show the same general results. In this instance the actual microscope readings are also recorded.

TABLE III

Observer A			*Observer A*			*Observer A*			
Collimated Line from Left			Collimated Line from Right			Collimated Line Alternately L & R			
30.0 30.3	30.2	+2.1	37.5 36.9	37.2	−0.6	31.8 31.3	31.6	+0.2	from L
27.0 27.5	27.3	−0.8	36.5 37.5	37.0	−0.8	37.5 37.2	37.4	−1.0	" R
27.2 27.2	27.2	−0.9	38.2 38.7	38.5	+0.7	29.2 30.0	29.6	−1.8	" L
27.6 27.9	27.8	−0.3	37.2 37.8	37.5	−0.3	39.5 40.5	40.0	+1.6	" R
29.2 28.9	29.1	+1.0	37.0 37.4	37.2	−0.6	32.1 32.0	32.1	+0.7	" L
27.4 28.1	27.8	−0.3	37.9 38.1	38.0	+0.2	37.3 37.7	37.5	−0.9	" R
27.2 27.5	27.4	−0.7	37.2 37.2	37.2	−0.6	32.0 32.0	32.0	+0.6	" L
						38.5 38.9	38.7	+0.3	" R
27.7 28.1	27.9	−0.2	37.9 37.8	37.9	+0.1	32.0 31.8	31.9	+0.5	" L
28.2 27.7	28.0	−0.1	38.0 38.4	38.2	+0.4	38.6 38.0	38.3	−0.1	" R
28.5 28.4	28.5	+0.4	39.0 39.0	39.0	+1.2	30.8 31.1	31.0	−0.4	" L
Means 28.1			37.8			31.4 L and 38.4 R			
Mean errors 0.68			0.55			0.70 L and 0.78 R			

TABLE IV

Observer	*L Direct*	*R Direct*	*Mean of L and R Direct*	*Mean of Alternating Readings*	*Direct Interval*	*Alternating Interval*
A	28.1	37.8	33.0	34.9	−9.7	−7.0
B	31.7	38.9	35.3	—	−7.2	—
C	31.4	38.6	35.0	33.7	−7.2	−6.6

The results are shown graphically in Fig. 34. Here the alternating readings are placed below the scale, and sorted out into readings approaching from L and from R; whilst the direct readings, one set L, and one R, are placed above. As before each line represents twice the m.e. of the set, and the midpoint is opposite that point on the scale representing the a.m. of the set.

The enormous difference between settings from the left and right is nothing to do with mechanical backlash, for the measuring instrument (the micrometer eyepiece of the reading microscope) was read the same way on all occasions, and was independent of the setting apparatus. Nevertheless, the phenomena are extremely like that of backlash, with one interesting difference. It will be noticed that the readings that came up from the left are on the right of the scale, and vice versa; thus indicating that the setting was invariably overshot, not undershot as in the usual case of mechanical backlash. This applies to Figs. 33 and 34.

On comparing these observations it will be noticed that the zero setting was changed between the two series of readings, and so absolute comparisons cannot be made; but it will be seen that, great as are the differences between different observers, they are easily swamped by the difference in the same observer when setting from opposite directions. Table IV summarizes the results of the three observers who took observations left and right. Only *A* and *C* took alternating readings, and they both show a slightly smaller discrepancy between L and R readings when alternating, coupled with a greater M.E. (see Table III for *A*).

Moreover, the mean of alternating readings L and R is not the mean of direct readings L and R for either observer; this probably only represents the natural uncertainty of any one observer when repeating a set after an interval, and is not otherwise significant. By comparing Tables II and IV (1st column), some idea of the wander of the observers can be obtained when repeating similar observations. Since the change in zero between the two occasions is not known, all that can be done is to make some assumption as to the relative position of the zeros in the two series, and thus to deduce the wanders of observers *A*, *B*, and *C*. There is one assumed change of zero which leads to the sum of the wanders of *A*, *B*, and *C*, being a minimum. This occurs if the change of zero from the 1st to the 2nd series is assumed to be +20.8 seconds of angle, and this makes the sum of the wanders of *A*, *B*, and *C*, 4.0 seconds; or an average of about 1⅓ seconds per observer. It is not claimed that this is the most likely zero change on the evidence (the least squares method would lead to that), but, if the zero change was not 20.8 seconds, then the average wander was necessarily greater than 1⅓ seconds.

Thus: Five observers performed optical settings of the type known as "symmetrical". The definition of the centre line was inferior. The resulting errors are due to elements having human, rather than the mechanical, restoring agencies. The various m.e. of the five observers varied from about three-quarters to two seconds for single readings; the mean m.e. being just over one second for single readings. If it be assumed that the errors in any one set approximately follow the accepted frequency law, then the m.e. of the sets varies from one-quarter to two-thirds of a second; the average being just over one-third of a second. Of all the five sets, not one had a mean value within one-half a second of the a.m. of the sets, and the average difference between the mean of a set, and the mean of these means, is one second, or just three times the m.e. of the sets. Assuming the frequency law, the chance of a set of readings being in error by three times its m.e. is about one in a hundred. Hence, the chance of five assigned sets having this error is:

$$\left(\frac{1}{100}\right)^5 = 1 \text{ chance in } 10{,}000{,}000{,}000.$$

This figure need not be taken too seriously; but clearly the chance is negligibly small if it be assumed that each set was aiming at the same "target."

Three of the observers repeated the same setting after a lapse of time (at least a meal or a night intervened), and the average difference, between the means of their two sets, could not have been less than one and one-third seconds per observer, and may well have been more. One and one-third seconds is four times the average m.e. of the sets—another most improbable event, assuming that each observer was shooting at the same target both times; (each observer is assumed to have his own private target this time, it is not necessary to suppose them all identical).

The observations so far summarized were all taken by bringing the collimated line into register from the left, but, immediately after each observer had finished the last mentioned set, he repeated the set bringing in the line from the right. Observers *A*, *B*, and *C* did this. The average m.e. of each a.m. was about one-fourth of a second, the worst observer *E* being absent, and the average difference between the sets L and the sets R was eight seconds, *and always in the same sense*. That is, the readings in which the line was moved from left to right were on the right of those taken from right to left. Again the theory of probability gives a likelihood of many billions to one against this event—at any rate it is wildly improbable; assuming, of course, that each observer was aiming at the same target when performing L and R settings (no assumption as to targets as between different observers). Finally two observers (*A* and *C*) alternately performed settings L and R, and the average difference between the mean of the readings L, and those R, was reduced to the, still enormous, gap of about seven seconds.

The acuity deduced from the mean errors of the sets works out at between nine and twelve seconds at the eye for observer *A* for his various sets, down to roughly thirty seconds for *E*. *E* was completely unskilled, but the mean acuity of the others (including *A*) was roughly sixteen seconds at the eye. When it is remembered that this really covers the scatter due to the microscope readings, and all instrumental errors, as well as the telescope-collimator setting, this acuity is not particularly poor even had the collimated line been of reasonably good quality.

The conclusions to be drawn seem to be as follows:

1. Each observer shows unmistakable signs of having a definite and reasonably constant mean for the duration of a set; "reasonably constant", that is to say, as compared with other inconstancies already noted. The observer's endeavor to maintain this mean constitutes the restoring agency.
2. On repeating the set after a time interval the mean has been found to change, but the precision of this set is about as before.

   This suggests that two separate metal acts are performed.

A. A judgment is made as to the character of the setting which best represents a symmetrical cut. This is not the same thing as saying that the setting is (subconsciously) judged to be symmetrical; for an inferior, fuzzy line does not look really symmetrical if examined carefully.[1] A poor line will have noticeable breadth and will not appear uniform, either as regards color, contrast (density), boundary, or the distribution of its elementary parts. The mind performs a judgment which amounts to solving this conundrum: what is the position of the ideal mathematical line with respect to this area which the latter is attempting to represent?

B. This decided, the observer then makes repeated attempts to set on this ideal line; this constitutes a different type of judgment to the former.

The curious thing is that an observer can make the second judgment nearly, though not quite, as consistently when the line is "ideal" as when it is "real" (i.e., working on a good line). This second judgment would seem to depend on some kind of visual memory, or it may be on a (more or less) subconscious memory of the relative weights which were assigned to the various considerations on which the first judgment was based. In the latter case it is these weights, and the nature of the considerations, which constitutes the first judgment; and setting agreeably to these weights and considerations involves the second judgment.

The point is that judgment (A) is necessarily antecedent to any setting, though it may be helped by trying out possible positions of the line (or object) to be set. This is done once for the set. A different sort of judgment (B) is then performed for every setting and consists in maintaining a restoring agency for the mean dictated by (A).

---

[1] Symmetry is never the result of equal carelessness, or lack of control, in two directions. All this could ever secure is an equal m.e. or liability to variation, on the two sides, with a scatter about this m.e. large in proportion to the lack of control. This leads, not to symmetry, but to an equal chance that the assymmetry is in either direction for any given instance.

A good telescope out of focus will give a symmetrically blurred image of a star; this is an instance of great control of parts, so devised as *not* to concentrate all the light from a star in the focal plane of the eyepiece. A poor telescope with indifferently fashioned lens surfaces will give an assymmetrical image of a star however focussed. Equal carelessness in making the two arms of a balance will not result in an unbiased instrument.

Judgment (B) appears to have an adequate restoring agency, as shown by the precision of the set. Judgment (A) is subject to wider fluctuations, as shown by the variation of any one observer repeating the same set of observations after an interval of time. This "A" variation is of the same order as that between five observers all performing similar sets. Consequently it seems as though all observers were aiming at the same mean in judgment (A), all with a rather poor, but very similar, restoring agency. And, very probably, this represents an incomplete element, in that no restoring agencies exist within limits which are really significant. The observation, however, lacks perfect symmetry, for the collimated line must be brought in from the left (as in settings just considered), or from the right; it cannot be deposited from nowhere. This leads to a systematic error in judgment (A) which easily swamps all other errors. When observers bring the line into register from the right they all perform judgment (A) with a bias to the left (i.e., overshooting), and vice versa. Thus, for judgment (A), the observers have two means, each common to the lot; one for each type of experiment. The experiment was varied a third way, by alternating L and R, and the only marked effect was to alter the bias in judgment (A) for lefts and rights for observers.

The curious point about this bias is that although a function of the *direction* of movement of the collimated line, it does not seem very sensitive to the *speed* of movement. No particular effort was made to keep the speed constant as between observers; and different observers usually have noticeably different setting speeds. Nevertheless the scatter as between observers is not greater than the scatter of one observer at different times.

There is no point in straining the resemblance between a mental act and a piece of mechanism, but it is relevant to notice that the same general principles apply to both; and the same sort of precautions, or lack of them, will prevent, or permit, erratic errors.

This analysis of the cause of erratic errors in the human element does not rest on the single illustration given, and the effects described have been repeatedly observed in connection with optical measurement on inferior objects; others who have done similar work have had the same experience.

From the designer's standpoint it is important to remember that an adequate restoring moment must be provided for a human event as for a mechanical one.

Two judgments enter into a human event: (A) What am I supposed to be doing? (B) Have I done it?

Judgment A (What am I supposed to be doing?) has all the appearance of not possessing adequate restoring agencies, and of exhibiting erratic errors in consequence, which are largely converted into systematic errors when assymmetry is introduced (i.e. bringing up the line from left or right). This is closely analogous to backlash. Judgment B (Have I done it?) suffers from far smaller short period errors.

A higher powered microscope (or telescope) or a greater sensitivity generally usually helps judgment (B), for it provides a greater restoring moment by showing a greater apparent deviation for the same actual deviation. But this device increases the difficulty of (A), for the greater the detail, the more difficult it becomes to sum up the situation. Since the errors due to (A) are usually the more serious, the obvious thing is to start by setting an easy problem (A), e.g., make the setting lines really uniform and definite, thus enabling the mind to act with conviction and an unalterable opinion. If possible the observation should be made symmetrical to eliminate bias, which acts as a force pushing the judgment in one direction against the restoring agency. Thus, in the above example, if practicable the observer should register the line from the right, and from the left, and take the mean; devices for doing all this in one operation are sometimes possible. In nine cases out of ten (B) is far better served than judgment (A), the existence of which is often not even realized. And since it is not always easy to unmask the errors due to (A), or know what sort of assymmetry does bias this judgment, it is particularly necessary to give the matter full attention.[1]

It was stated above that sufficient evidence existed roughly to evaluate the comparative weight to be attributed to the errors of readings due to the telescope setting, and to the more accurate microscope setting. This can be done by the help of Table II, where the actual microscope readings are given; these are seen to have been taken in pairs, and the difference between any two members of a pair, without respect to sign, constitutes a measure of the scatter of these readings (not their m.e. of course, which is equal to:

$$\frac{\text{mean scatter}}{\sqrt{2}},$$

according to the accepted Law of Errors). By arranging the final readings also in pairs, a similar measure of their scatter is obtained. This is shown in Table V for the first two columns of Table III.

Since the mean of two microscope readings is associated with one observation, the relative scatter due to the microscope in each observation is,

$$\frac{0.36}{\sqrt{2}}=0.26 \text{ second.}$$

If x represents the scatter due to the telescope setting,

$$0.78^2=0.26^2+x^2.$$

Hence

$$x=0.74.$$

---

[1] This discussion has centred round one particular type of setting; one line laid directly upon another, but the same considerations apply with equal force to any of the alternatives (e.g., two parallel graticule lines centred about the object line; a graticule "cross" symmetrically cut by the object line, etc.).

TABLE V

Microscope Readings. Seconds	*Scatter Seconds*	*Corresponding Observation. Seconds*	*Scatter Seconds*	*Microscope Reading. Seconds*	*Scatter Seconds*	*Corresponding Observation. Seconds*	*Scatter Seconds*
30.0 30.3	0.3	30.2		37.5 36.9	0.6	37.2	
			2.9				0.2
27.0 27.5	0.5	27.3		36.5 37.5	1.0	37.0	
			0.1				1.5
27.2 27.2	0.0	27.2		38.2 38.7	0.5	38.5	
			0.6				1.0
27.6 27.9	0.3	27.8		37.2 37.8	0.6	37.5	
			1.3				0.3
29.2 28.9	0.3	29.1		37.0 37.4	0.4	37.2	
			1.3				0.8
27.4 28.1	0.7	27.8		37.9 38.1	0.2	38.0	
			0.4				0.8
27.2 27.5	0.3	27.4		37.2 37.2	0.0	37.2	
			0.5				0.7
27.7 28.1	0.4	27.9		37.9 37.8	0.1	37.9	
			0.1				0.3
28.2 27.7	0.5	28.0		38.0 38.4	0.4	38.2	
			0.5				0.8
28.5 28.4	0.1	28.5		39.0 39.0	0.0	39.0	
Mean scatter	0.34	Mean scatter	0.86	Mean scatter	0.38	Mean scatter	0.71

Mean scatter for all microscope readings 0.36 second
Mean scatter for all telescope readings 0.78 second

Thus rounding off these figures;

Scatter of observation $=0.8$ second.
Scatter due to microscope $=0.2_5$ second.
Scatter due to telescope $=0.7_5$ second.

Other observations made by *A* on the same work (not given here) bear out these figures approximately. Thus about three-quarters of the whole scatter was due to the telescope setting. As regards systematic and erratic errors, the former do not arise since only differences were required from the microscope. The microscope "cut" represented a far better optical system than that of the telescope; it was always read with the parts moved in the same sense, and, since the judgment (A) "What am I supposed to be doing?", was much easier, erratic errors are likely to be relatively better than the scatter as compared with those due to the telescope.

It is interesting, in Table VI, to compare the largest residual in each column in Table II, and also the first two columns of Table III with their respective mean errors; to these have been added all the other sets of ten similar observations taken at the same time but not given here; "alternating" sets have been omitted in all cases but otherwise no selection has been made. Column 3 shows the approximate probability, of any one reading in the various sets of ten, having a residual as great as the actual largest residual, as calculated by the accepted Law of Errors.

TABLE VI

M.E. of Set	*Largest Residual in Set*	*Probability of Any One Reading Having So Large an Error*
0.76	2.1	0.03
1.22	3.9	0.01
0.96	2.3	0.06
0.72	1.5	0.10
1.97	3.6	0.14
0.68	2.1	0.01
0.55	1.2	0.08
1.04	2.8	0.03
0.70	2.2	0.01
1.65	2.7	0.20
0.72	2.2	0.01

Since each set contained 10 observations as many sets should contain maximum residuals with a probability above

0.10 as below this figure. Out of 11 sets in only 3 has the largest residual a probable frequency above 0.10 whilst 8 are below. Moreover in 11 sets a residual with a probable frequency of 0.01 should only occur about once, actually it has occurred 4 times. This illustrates the habitual excess of "huge errors" which is the common experience of experimenters.

Finally, it may be asked, what is the evidence that the residuals of the sets of observations are samples of a population obeying some law sufficiently allied to the accepted Law of Errors to make the procedure proper to the latter applicable? This particularly applies to the propriety of accepting the a.m. as the probable value of the set and deducing its m.e. from that of the observations composing the set. It has already been seen that "huge errors" are in excess, but it has been shown earlier that the correspondence between the actual and accepted laws of frequency will be weakest at just this point, and that this does not materially affect the bulk of the readings.

To test this correspondence the moduli of all the sets, whose m.e. is given in Table VI, were obtained by multiplying their individual m.e. by 1.77, and the residuals of all readings in these sets were expressed as a fraction of the modulus C of the set. All residuals of all sets, so expressed, are then comparable. Two types of bias might be expected; first that observers tended to have residuals of different magnitude as between their right and left sides; this might be expected if the fuzzy collimator line had a very different character on its two sides; secondly that, since the observation involved bringing up the collimator line from one side or the other, the scatter might be expected to be different on the "short" and "over" sides of the a.m. (this has nothing to do with the very heavy bias of the a.m. due to this assymmetry, which has already been noticed).

To get some line on these possible biases the residuals of the sets $\left(\text{expressed as } \frac{\epsilon}{C}\right)$ were combined in two ways. In the first way, those residuals were considered positive which represented a deviation to the left of the observer, irrespective of which way the cut was made; and then the residuals were divided into groups, depending on their size and the number in each group plotted against their size: Fig. 35(A). Secondly all those residuals were considered positive which represented an "over", having regard to the direction in which the collimator line was brought into register, and regardless of the observer's "hand"—these residuals were grouped as before, and plotted: Fig. 35(B). In both figures the accepted frequency curve which these residuals are supposed to be obeying is shown.

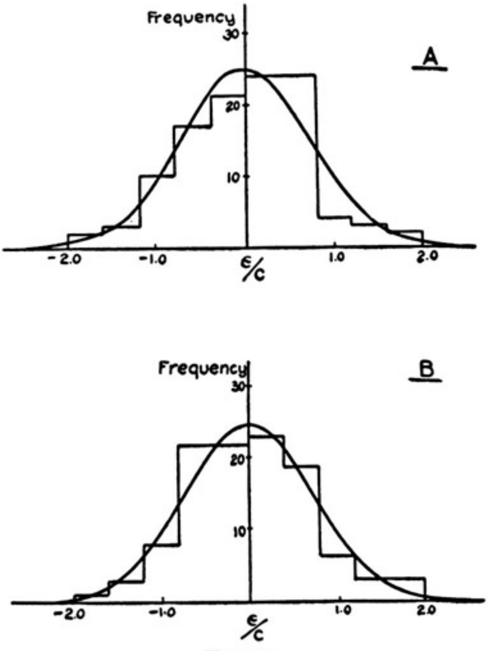

Fig. 35.

Considering the very small number of observations involved (only 110) the fit is quite good, in the sense that both actual curves are rough shots at the theoretical curve, and show no very significant assymmetry. Hence it can be concluded that the a.m. of the residuals of a set of such observations is a likely value of the quantity indicated by the elements with restoring agencies as existing at that time; and further that roughly

$$\text{m.e. of a.m.} = \frac{\text{m.e. of one observation}}{\sqrt{n}};$$

where n represents the number of observations in each set. At least, this is roughly true, provided n is not so large as to bring down the m.e. of a.m. to a figure comparable to the assymmetries which doubtless do exist in the scatter of these observations.

Of course this extra precision is useless and even misleading in the present case, for the systematic errors were found to be altogether larger than the scatter in any one set, and, unless these systematic errors are the very quantities to be measured, there is no point in taking more than two observations in each set—and then only because two observations show up a gross mistake.

Incidentally the evidence just considered seems to show that the relation between sensation and stimulus does not follow a logarithmic law in this instance. If the resultant disturbances surrounding the observation follow the Law of Error, then, over the range tested, the change in sensation appears to vary roughly as the change in stimulus.

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PART II

DESIGN

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CHAPTER VII

PRECISION—KINEMATIC DESIGN

GENERAL

It has been shown that precision, the elimination of erratic and short period errors, results from securing that all the elements involved in a measurement are complete and stiff; and an element has been defined as complete when its component links have the correct degrees of relative constraint.

The meaning of "freedom" and "constraint" in their geometrical sense is well known to designers and scientists generally, and will be very briefly recapitulated; this amounts to nothing more than a theorem in the geometry of three dimensions. In this sense it is an abstraction of reality, and, like all abstractions, only represents part of the actual facts. The next step is to consider the modifications or limitations imposed on these ideas, as applied to actual matter, such as is used to form instruments and mechanism generally. It will be seen in particular that the notions of freedom and constraint are unduly simplified as applied to mechanism, and the antithesis is not nearly so sharp as may appear at first sight. Having thus come to an understanding as to the character of a complete element, the remainder of the chapter will be devoted to a consideration of the best means to ensure that elements shall be complete in virtue of their design, and with only reasonable demands on workmanship.

DEGREES OF FREEDOM

A point in space is said to possess three degrees of spatial freedom, corresponding to the three dimensions of space.

What is meant is that, relatively to some arbitrary spatial framework, a point has three possible directions of motions, such that a movement in any one of them does not affect the position or motion of the point as measured along the other two directions.

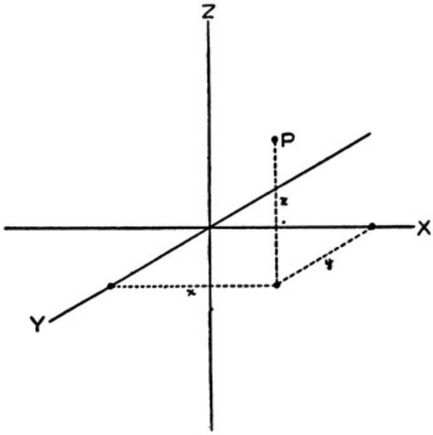

FIG. 36.

A convenient framework consists of three axes all mutually intersecting at right angles at one point, and, as every one knows, this is the usual convention. Thus in Fig. 36 the coördinates of the point P, with reference to the framework, or axes, XYZ is (x, y, z), and it is clear that P can move parallel to any one axis without changing the coördinates relevant to the other two.

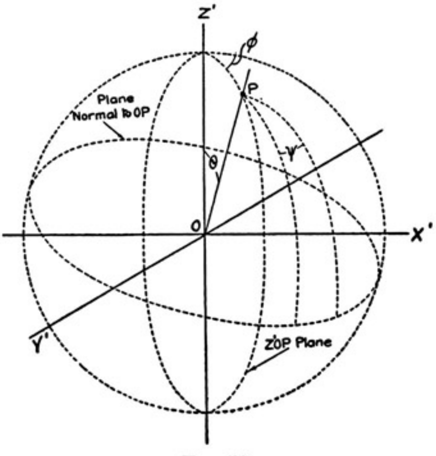

FIG. 37.

If (x, y, z) be the coördinates of some given point in a body, the latter is capable of being rotated through any angles θ, ϕ, ψ, about three directions without changing the position of that point; and a change in the value of any one of these three angles does not involve any change in the others; hence a body has six degrees of spatial freedom, three of translation and three of rotation, relative to the given axes. Thus six independent quantities (x, y, z, θ, ϕ, ψ) are required to fix the position of a body with respect to any set of axes. In Fig. 37, suppose some point O has the coördinates (x, y, z) with respect to some set of axes, and that axes x', y', z' are drawn through O and parallel to the chosen axes. Then any given line through O, OP say, has its direction determined by the two angles θ and ϕ; where θ is the angle $Z'OP$, and ϕ the angle between the plane $Z'OX'$ and $Z'OP$. Conceive OP as a given line in a body, fixed with respect to the given axes so far as OP is concerned. The body still has one degree of freedom left, and its final position is determined by a simple rotation round OP through the requisite angle ψ.

This, of course, illustrates only one of a number of possible types of spatial frames, relatively to which the position of bodies can be uniquely determined; and each system leads to a different expression of position; but they all agree in this, that a body has six independent possibilities of motion in a three dimensional space, such that any one of these motions does not involve the rest. This is an intrinsic property of three dimensional space, and so is independent of any arbitrary assumptions as to frameworks or constructions of any kind.

It follows immediately from what has been said that any one body has six degrees of freedom with respect to any other body, for the axes of reference can always be conceived as being at rest with respect to one of the two.

SOLID BODIES

So far we are in the domain of pure geometry—the science of space—for our "body" is nothing more than a collection of mathematical points, having invariable relationships to each other, that is "rigidity", but possessing none of the exclusive properties of matter. The first step towards actuality is to endow these "geometrical" bodies with one of the characteristic properties of matter, namely solidity, by which I mean that quality of exclusiveness which prevents two pieces of matter from occupying the same position at the same moment; this leads to the ideas of a limited degree of freedom, and of constraint.

For instance consider two solid bodies, with the shapes of a pea and a bottle, having of course six degrees of relative freedom; either can, in theory at least, occupy any position irrespective of the other; but, if the pea be put inside the bottle and the latter be corked, their relative movements are severely limited, although they still possess six degrees of relative freedom. A little reflection will show that these bodies still possess unlimited angular freedom, but only a very limited freedom of translation in any direction. Had a stick been placed in the bottle, just too long to lie across the latter, two degrees of angular freedom would also be limited.

Thus a degree of relative freedom between two solid bodies may be limited or unlimited; and erratic errors are usually, though not always, the result of extremely limited degrees of freedom which should not exist.

DEGREES OF CONSTRAINT

The opposite of freedom is constraint. If two solid bodies are compelled to move in such a manner that they have no longer six independent relative motions then they are said to be constrained; thus two bodies with four independent relative motions have four degrees of freedom, and two degrees of constraint. This is not, of course, equivalent to saying that any of the six quantities x, y, z, θ, ϕ, ψ are constants; they may all be capable of change, but no longer are they all independent. Thus, in the case of two bodies with four degrees of relative freedom, two of these quantities are functions of one or more of the other four; hence, knowing the functions involved, the value of the dependent variables can be stated in terms of the relevant independent variables, and the relation of the two bodies is completely determined so soon as these four are given. A special, but very common, case occurs when one or more of the dependent quantities is incapable of change, and so becomes a constant.

The only case in which the constrained movements are necessarily reduced to zero is when two bodies have no degrees of relative freedom, in which case they behave as one and are incapable of relative motion. A simple example may help. A sleeve A, Fig. 38(A), fits the endless rod B, and has two degrees of freedom with respect to B; for it can independently rotate and slide on the latter. The four dependent motions, in this instance, are completely prevented by the shapes and positions of the two bodies. Consequently, in order to determine the relative positions of A and B two quantities, one length, and one angle, are required. In Fig. 38(B), A' represents a nut engaging the endless screw B', and the former can still rotate and move along the latter, but these motions are now strictly related, and a knowledge of either enables the other to be calculated; consequently A' and B' have only one degree of relative freedom, and it is a matter of indifference whether the free motion be taken to be the rotation, or the linear displacement. Hence, of the five degrees of constraint existing between A and B, four preclude any change of their respective quantities, whilst the fifth constraint relates the two remaining motions.

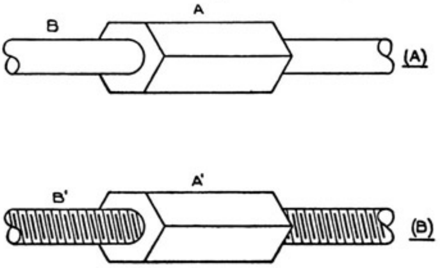

FIG. 38.

If, Fig. 38(A), the rod B were of finite length, ending in stops, A and B would have unlimited angular freedom of one degree, but only limited freedom of translation; if B', Fig. 38(B), were similarly provided with stops both degrees of freedom would be limited. Finally, if the two stops on the rod or screw approached each other, until they only just left room for A and A', in the former case only one degree of freedom would remain, and in the latter none.

CONSTRAINT OF MATERIAL BODIES

So far the bodies considered have had no material properties except that of solidity; but now they will be considered as ordinary matter with all its characteristics, and in particular those of imperfect rigidity, weight, and friction when two bodies are in contact.

As soon as we consider real bodies the conception of "constraint" given above will require modification; for no two bodies can be so held that absolute obedience to any law governing their relative position can be expected in the presence of disturbing causes; all elements are liable to disturbance. At the other end of the scale no two bodies on this earth are absolutely unconstrained, directly or indirectly, for, by virtue of their mass, all bodies are under the influence of gravity, and if left to themselves will tend to rest in some relation to each other. This in fact is the usual constraint in the case of matter in its natural state, such as stones and mountains; and appears to be adequate to maintain great immobility in the latter case.

It is sometimes suggested that two components of a mechanism have such and such degrees of constraint when they are so constructed that these constraints cannot be overcome without breaking the instrument; but this does not begin to cover ordinary practice, unless indeed the instrument be defined as broken when the constraints prove inadequate; but this is mere tautology. Thus gravity is often used as the sole force for keeping some component in place, or perhaps a spring which, by the application of an adequate disturbing force, would permit considerable movement without in any way permanently altering the various parts.

A galvanometer may be said to have six degrees of freedom with respect to the stone slab on which it rests, in that it can be moved anywhere without change of form; but nevertheless it is assumed that, subject to obvious precautions, the galvanometer and slab will behave as though they had no degrees of relative freedom, even in the presence of limited disturbing forces such as slight vibration or currents of wind. The weight of the galvanometer represents the constraint in the case of one degree of translation and two degrees of rotation; but, so far as weight alone is concerned, the instrument is free to slide about its slab in two dimensions and rotate about a vertical axis; these three motions are constrained by the presence of friction between the legs of the instrument and the slab, the constraining force being,

$$\mu W.$$

Hence, relatively to certain conditions, the galvanometer and slab are effectively constrained, but in the presence of others (e.g., the inevitable charwoman who dusts away the previous day's work) some constraints are evidently lacking.

A more resistant system of constraints is the well known "hole, slot, and plane". In the presence of considerable movement and shock these constraints are no longer effective, and gravity must be replaced or reinforced by some more adequate force before an instrument could be considered effectively constrained.

DEFINITION OF CONSTRAINT

Thus it is seen that freedom and constraint are relative terms, partly depending on the magnitude of likely disturbing causes. Two bodies will be considered to have a possible degree of freedom constrained when one of the two following sets of conditions is satisfied; the first relating to restored elements, and the second to maintained elements.

(a) Two bodies forming the links of a *restored* element have a degree of possible freedom constrained when a unique relation between the two bodies is indicated by restoring agencies, such that, in the presence of the internal friction of the mechanism, this relation will be restored to within significant limits when external disturbing causes are removed. Further the likely disturbances of that element must not be such as to prevent an observation from having the required accuracy; i.e., the constraints must be sufficiently stiff.

(b) Two bodies forming the links of a *maintained* element have a degree of possible freedom constrained when a unique relation between the two bodies is held by maintaining agencies as regards the motion in question, such that the relation is not significantly disturbed throughout its required duration by any likely combination of disturbing causes.

These definitions of constraint in restored and maintained elements require an explanation, especially with regard to the last sentence in the former. This states that a body is not constrained merely because an element is indicated, but that the indication must, in addition, be sufficiently stiff to secure the desired accuracy in the relevant observation. Suppose, for instance, that the direction of the earth's gravitational pull be required at some particular place; one method of proceeding would be to suspend a small mass at the end of a fine wire, and to note the direction of this wire. This, in the absence of disturbing causes, should be the required direction at that place. In the presence of draughts the plumb line will be continually disturbed; and, since the direction of the draughts is more or less constant, the resulting short period errors are biased. Assuming that the wind is reasonably light the plumb line will give a builder the information he requires with the desired accuracy, and the indicated element (plumb line, earth) is adequately stiff. Imagine, however, the observer to be a scientist, engaged in an observation of high accuracy; quite clearly the draughts will entirely spoil the observation, in spite of the fact that the restoring agency (the earth's gravitational pull) never ceases to indicate the correct element. Hence adequate stiffness is an essential factor in constraint; though it is not possible to give a precise meaning to the word "adequate" except in particular cases, and in relation to all the special circumstances.

Thus, an observation consisting of four elements, *A* to *D*, is required to have an error not exceeding 10 units of measurement. If *A* were not burdened with error, the error of this observation would be 4 units; actually *A* accounts for 8 units and the final error has the inadmissible value of 12. Evidently the error due to *A* alone is admissible, as are those due to *B*, *C*, and *D*, without *A*. It is quite impossible to state a priori which elements are underconstrained due to inadequate stiffness. Investigation might show that all the elements could be enormously improved in this respect, or that none of them were susceptible of improvement, and that a lower accuracy must be accepted; in either of these cases all that can be said is that the stiffness of the elements taken as a whole is inadequate. Alternatively it might appear that element *A* could most easily be improved, or, on the contrary, that elements *B*, *C*, and *D* are alone susceptible of being rendered sensibly accurate; in the first case it would be more useful in practice to regard *A* as inadequately stiff, whilst in the last case *B*, *C*, and *D* would be at fault. Thus when an observation as a whole lacks stiffness it is not necessarily that element with the greatest excursions which is incomplete in any useful sense, and the matter must be threshed out by common sense, rather than by appeal to definition.

OVERCONSTRAINT

The word "constraint" is used sometimes with a different significance to that already discussed; thus two bodies are said to be overconstrained when a particular relative motion is prevented in more than one way, e.g., a rotating shaft requires to be held in not less than two journal bearings which define the axis of rotation. If the shaft be fitted with three journal bearings A, B, and C, these define three axes;

$$AB.$$
$$BC.$$
$$CA.$$

Unless the workmanship is perfect these three axes would not be coincident, and, even if they were, one bearing is kinematically redundant and the bodies (shaft and bed) are "overconstrained".

These two uses of the word constraint, as referring to a *degree* of freedom prevented, and also to the *number of particular means* by which a given degree is prevented, is so firmly intrenched in the language of kinematics that it is useless to protest, but the two should never be confused.

POINT CONTACTS AND CONSTRAINT

It is easily seen that, in the absence of friction, the minimum number of point contacts required to constrain two bodies is equal to the degrees of constraint imposed. Thus, neglecting friction, a ball on a level table has one point of contact, and one degree of constraint (that of translation vertically); two wheels rigidly attached to one shaft on a level plane have one degree of translation, and one of rotation, constrained, with two points of contact; neglecting friction again. Similarly a tripod on a table has three points of contact, and three constraints. A cylinder supported on two V's has four points of contact, and four degrees of constraint; one rotation and one translation remaining free. The same solid cylinder, having an end face pressed on to a point fixed with respect to the V's, has fine point contacts, and only one rotation remains free. If a crank on this cylinder rests with a side face on one point, in addition to all previous contacts, it is fully constrained, with six point contacts.

In these examples each constraint prevented all movement in one direction, but, as already seen, a constraint may only reduce what was an independent movement to a function of some other movement. Thus, in the case of the cylinder supported on two *V*'s, two independent motions remain, a rotation of the cylinder round its axis, and translation along it; but suppose a thread be cut on a short length of the cylinder, and one arm of one *V* be fitted with a small ball, so placed as to engage the thread, with, of course, a two point contact; then the translation and rotation of the cylinder are related, and five point contacts provide five constraints.

By suitably rearranging the required contacts between two bodies, the resulting degrees of constraints can be made to constrain alternative motions; thus five point contacts can be made to leave any assigned motion of translation or rotation free; and, where two or more motions are strictly related, it is a matter of convenience which shall be considered the independent variable. But not all combinations of freedom and constraint are in fact possible as between two bodies, and solely in virtue of their form;[1] e.g., two or three degrees of translation involving no rotation.

FORCES AND LOCATORS

It has been assumed that some force, or forces, are acting to render each contact or "locator" effective, and the nature of these forces requires consideration. In some cases the two bodies are so positioned that gravity acts in the required direction, and, in the absence of great friction or severe disturbing causes, makes the constraints effective; alternatively a spring, or springs, magnetic attraction, a force consequent on acceleration, etc., are used on occasion to maintain the necessary contacts. Some of these forces act on the whole volume of the body, others on the surface, and yet others at a point; but they all have one characteristic in common; namely, none of them taken alone define a position for the body on which they act, their spatial characteristics being direction and sense, but not position.

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[1] Of course any combination can be secured by intermediate linkwork.

Thus a "locator" consists of a close approximation to an unyielding point contact which defines a position; with a "force" effecting this contact, whose magnitude is roughly constant for all likely displacements of the two bodies.

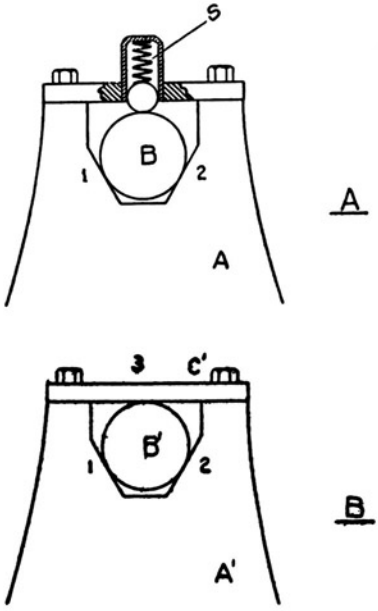

FIG. 39.

No material of construction is completely rigid or lacking in elasticity, and any locator can be regarded as extremely stiff spring with an abnormally small range; it is the object of design to secure that this range is below the significant level, in order that the relative position of the two bodies may be sensibly unique for a given setting of the independent motions. Hence it is unreasonable to expect the same sort of contact to complete the constraint. Thus, Fig. 39(*A*), a rod *B* is constrained by a *V* and a spring *S*; the latter serving to effect the two point contacts. Figure 39(*B*) shows the spring *S* replaced by the cross bar *C'*, resulting in a third locator 3; so called because it determines the position of *B'* to the same extent as 1 and 2; this results in forces between the parts which are difficult to control, owing to the stiffness of all the members, and which varies widely as a result of very minute dimensional changes.

It is seen that a locator is not so called merely because physical contact exists [e.g., the plunger under the spring S is not a locator, Fig. 39(A)], but because, in addition, the force with which the contact is held varies so rapidly, with relative change of position, as sensibly to determine a relative position [e.g., locator 3, Fig. 39(B)].

The distinction between a *locator* and an effecting force is one of degree only, in the absence of perfectly rigid material; but in practice the difference is so great as to be fundamental.

OVER AND REDUNDANT CONSTRAINT

A page or two earlier, a shaft, held in three journal bearings, was given as an example of overconstraint (i.e., a possible degree of freedom constrained in too many ways). Another example, often quoted, is that of a four-legged instrument. The four legs, A, B, C, and D define four planes;

$$ABC.$$
$$BCD.$$
$$CDA.$$
$$DAB.$$

In general these planes will not be coincident; hence the instrument and table are usually said to be overconstrained. But there is an important difference between these two examples, for, in the former case, one locator is rendered effective by another locator, and if necessary the shaft is forcibly bent to achieve this; whilst in the latter case the effecting agent is a force (gravity), and is small in the usual run of fairly light instruments, hence nothing compels the redundant locators to be effective—in other words, however many legs the instrument may have, the chances are it will only use three. This results in uncertainty of position rather than of force, whilst the former example results in a possibility of both.

The term *overconstraint* should be reserved for the case where one locator is rendered effective by another locator; and *redundant constraint* should be used when the number of locators are redundant, but when each is rendered effective (if at all) by an effecting force.

KINEMATIC DESIGN

Kinematic design, as that term is frequently used, implies a design whereby the various links of each element, forming an instrument, are constrained by the theoretically minimum number of point contacts, given the degrees of constraint required. The claims for such a design are so frequently overstated that it will be as well to consider its very real merits, and its limitations.

In the first place, no two pieces of real matter ever have, or can make, contact at a single point; in fact when it is remembered that atoms are not "in contact", but, on the contrary, represent the foci of fields of attraction and repulsion, the very conception becomes nonsense. Moreover the finest material point involves countless atoms, and is an area as seen under a microscope. However, when a hardened ball and plane are in contact, the area involved may be thought of as a "point" without serious error, so long as the problem is geometrical. Apart from this approximation elements sometimes are designed kinematically; this especially applies to small light parts having to transmit small or negligible forces; for the surfaces involved in "point" contacts are exceedingly small, and, if the force between the links is great, the specific pressures become enormous, with consequent wear and destruction of the working surfaces.

On the other hand, the very fact that a constraint is restored by a force, whose magnitude is approximately independent of the relative position of the two links (over distances covering reasonable limits of manufacture or disturbances), enables that force to be kept comparatively light. For, in the case of overconstrained links, it is natural to provide tight fits (i.e., relatively great forces), in order to ensure that no play exists; these forces are extremely difficult to control, depending as they do on minute changes of form, and in the case of moving parts, e.g., a frame or saddle sliding between gibs, the force is apt to vary greatly from one part to another unless the workmanship is of the very highest quality, and therefore price. Even in this case the force is very sensitive to oil and dirt, and unfortunately the first tends to collect the second. Since the friction force between two parts is roughly proportional to the total pressure between them, kinematic design, by reducing this pressure, relieves the mechanism of the greater part of its friction forces, and so secures smaller disturbance to elements; making it easier to ensure that these are complete, besides lightening the component parts. This effect is cumulative in a train of elements and is of the greatest importance.

Not only are the forces lighter in the case of a kinematically designed element but their position is accurately known. In Fig. 40(*A*) and (*B*), two methods of arranging a slide are shown; the first of which has the minimum number of constraints, with gravity as the force of constraint; whilst the second method is borrowed from machine tool practice.

In Fig. 40(*A*) the slide *S* is provided with four semispherical projections, 1 to 4, which ride on the circular rod or hollow cylinder *R*, whilst a fifth projection makes contact with the flat bar *B* and, in the presence of gravity, these five point contacts permit of one movement only, that of translation parallel to *R*. The positions of contact are all known, and it is easy to calculate the disturbances to be expected, in the movements of the slide, from any given irregularity in the surfaces of *R* or *B*, or their lack of parallelism in a vertical sense. Arbitrary figures can be postulated for any or all these faults, corresponding to reasonable methods of manufacture and adjustment, and it is at once seen whether the resulting disturbance of the slide is significant having regard to its duty. If the resulting error is not negligible the element can be redesigned, or, if practicable and desirable, better workmanship may be called for. In any case the designer can see what he requires, and is likely to get, in a few minutes.

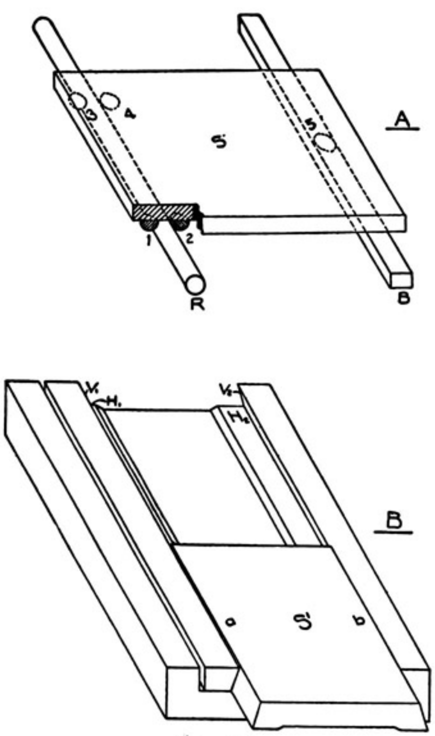

FIG. 40.

Figure 40(*B*). Here, assuming an extreme case, the slide *S′* bears, or is intended to bear, over its whole length on two horizontal surfaces H_1 and H_2, and also on two nearly vertical faces V_1 and V_2. This is an impossibility unless H_1 and H_2 present the same relationship to each other throughout their entire length, and are the counterpart of the under surfaces of *S′*; whilst the same remarks apply to V_1 and V_2, only in this case their distance apart is also involved. All these conditions can only be very approximately satisfied; and even if typical errors are postulated, not an easy thing to do, it is nearly impossible to predict what will happen to *S′* as it slides. For instance, if the slide happens to be pinched at *a* and *b* between the gibs, however stiffly it is held the slide is liable to a wobble, i.e., a small rotation about a vertical axis; in fact the more stiffly the slide is held, the more likely is any attempt to move it to result in this very rotation. It is usual in such a case to relieve the centre portion of these long sliding surfaces on S', and this undoubtedly secures that it is held, if at all, at its ends; but even so the probable disturbances are quite incalculable, and by no means always small. This is typical of an overconstrained element, and, as a further defect, in an attempt to complete the element, S' will probably be strained and so be acting as an impromptu spring.

Another instance, in which it is not always easy to constrain an element correctly, is the case of a slide actuated by a micrometric lead screw carrying the recording drum. The problem is to secure that the position of the slide accurately corresponds to the angular position of the screw, without overconstraint of either link. If the total movement required is small, say 4 or 5 centimetres, the screw can work in a fixed nut and butt against a hardened surface on the slide [as in Fig. 41(*A*)]; or the connection, shown in (*B*), may be used to reduce the lateral disturbance on the slide due to any decentring of the screw point. In either case the slide

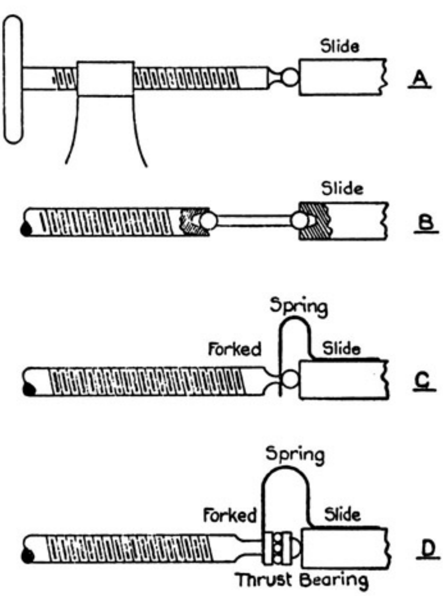

FIG. 41.

must be returned by means of a spring; and, if it connects the slide to the frame, the spring must be long in comparison to the travel, in order to prevent serious changes of force; the spring might connect the screw head to the slide, as shown in (*C*) and (*D*); the former might result in considerable lateral forces on the slide due to friction, whilst the latter would require some sort of guard to prevent the two halves of the thrust ball bearing coming apart and dropping the balls, in the event of the spring pressure being temporarily relieved.

If the lead screw be fixed, except as regards rotation, the nut travelling instead, the problem is more complicated and I know of no complete solution which is really simple; the difficulty, of course, lies in arranging for the nut to be constrained in only one translation with a high degree of precision, in combination with the constraint of one rotation with respect to the slide.

Figure 42 shows three ways of using a hardened steel ball (as used for ball bearings) to clamp a rotating link without any likelihood of erratic errors, even if roughly made. It will be realized that in all cases the surfaces contacting with the ball should be hardened. The sketches are largely self-explanatory; the advantage of (*B*), as compared with (*A*), lies in the absence of heavy thrusts on the rotating and stationary links. (*C*) differs from either in that the clamp is positive instead of depending on friction, and so a given setting of the two links can always be accurately repeated; on the other hand only a limited number of such settings are possible. In the last example that link (whether rotating or stationary), which carries a number of *V*'s corresponding to the settings required, is responsible for the interval between these settings; hence, to facilitate the accurate machining involved, these *V*'s are cut parallel; the single *V* on the other link being tapered. This latter *V* should be given some adjustment sideways if the absolute,

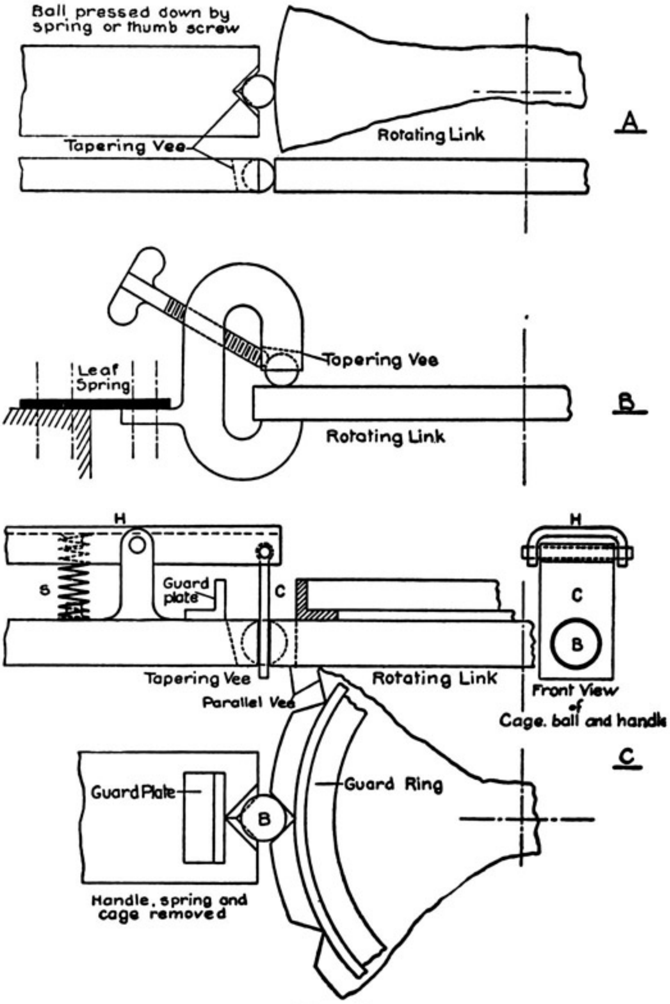
Ball pressed down by spring or thumb screw
Rotating Link
A
Tapering Vee
Leaf Spring
Tapering Vee
Rotating Link
B
H
S
Guard plate
C
H
C
B
Tapering Vee
Parallel Vee
Rotating Link
Front View of Cage, ball and handle
C
Guard Plate
B
Guard Ring
Handle, spring and cage removed

FIG. 42.

as well as the relative, positions of the settings are critical.

In the sketch the rotating and stationary links determine the relative and absolute positions of the settings respectively. To release the setting the handle *H* is depressed against the compression spring *S*, raising the cage *C*, and so withdrawing the ball *B*. The movement of *H* is limited so that the tail of *C* never comes out of the gap between the two main links; whilst the guard plate and ring prevent the ball from leaving its cage. On releasing the handle the spring *S* forces the ball down so soon as the opposing *V*'s come into register.

It will be noticed that the ball makes contact with four locators in example (*C*), whilst the cage represents the effecting force. Three links or components are involved, the rotating and stationary links and the ball. The two former, when locked, have one degree of freedom suppressed, whilst the ball has its three degrees of translation constrained; thus, in all, four degrees of freedom are suppressed, which corresponds to the number of locators involved. Incidentally one force effects all four constraints. These three examples do not, of course, begin to exhaust the possible arrangements of this type of clamp; thus example (*C*) could be arranged somewhat on the lines of (*B*) if desired, relieving the rotating and stationary members of side thrust.[1]

SUMMARY

The chief advantage of kinematic design can be summed up as follows:

(a) Since each element is constrained with the minimum contacts, rendered effective by forces which do not define a position, the forces acting at each contact are known, and can be very easily controlled; this makes for light forces, small friction, complete elements, and so predictable behavior.

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[1] Some of the most beautiful examples of kinematic design are to be found in the instruments designed and made by the Cambridge Instrument Company of Cambridge, England, and these repay careful study; failing the actual instruments much can be learned from their illustrated catalogues. I know of no other makers of scientific instruments who so unfailingly use kinematic design on every possible occasion.

(b) Since the forces and friction are small, disturbances due to the elasticity of the material composing the links are usually below the significant level; consequently significant departures are restored by agencies whose moments are finite, and relatively large, for disturbances on the threshold of significance, and the element remains complete even in the presence of moderate friction.

(c) The completion of an element does not depend on accuracy of dimensions within wide limits, and hence is not jeopardized by wear in working surfaces.

(d) In the case of instruments designed to take differential measurements, the resulting precision often becomes a guarantee of absolute accuracy (subject to surface inaccuracies) even in the case of second-class workmanship; in any case the errors will not be erratic and can often be discounted.

(e) Wear is often greatly reduced as a result of the smaller forces and lighter parts involved, though this is partly discounted owing to the extremely small bearing surfaces involved, and is quite untrue unless the forces involved are mainly those of constraint and internal friction.

This brings us to the two great disadvantages of kinematic design, namely, the very small working or bearing surfaces, and what has been described as "flabbiness" in the face of disturbances. The last point is denied by some enthusiastic supporters of kinematic design, but it is, I believe, the universal experience of practical designers whose business it is to produce instruments subject to heavy disturbances, such as nautical instruments, etc. In these cases a compromise is usually sought; and I shall endeavor to suggest one or two leading principles by which a "semikinematic design" can be made more resistant without losing all the advantages secured by using only a minimum of constraint. But perhaps a word is desirable to explain more clearly just what is meant by "flabbiness", of lack of stiffness, of the elements in this connection; for the summary just given might seem to be a direct contradiction of this.

What has been claimed for kinematically designed elements is that the restoring moment, for rather slight disturbances such as occur in laboratories, is very good compared to the older types of design. On the other hand, the generous bearing surfaces and redundant locators of the latter permit of such instruments standing more gross abuse before the components literally part company, or at least completely fail to function. The restoring moments of such instruments are often nearly zero for very small significant disturbances, but rise to enormous values for great disturbances since locators are rendered effective by more locators.

In the case of kinematically designed instruments the effective forces are relatively light, to reduce friction and wear on the very small locating surfaces, and so although the restoring moments for small significant disturbances are good, these moments do not increase greatly as the disturbances become more violent.

CHAPTER VIII

PRECISION—SEMIKINEMATIC DESIGN

SEMIKINEMATIC DESIGN

The qualities to be retained, so far as possible, are small stresses, light friction between moving parts, complete elements in the presence of dimensional errors and of dimensional changes due to wear, etc., and predictable behavior.

The number of ways in which the constraints between two bodies may fail to be kinematically applied are legion but they can be grouped under a few headings.

1. The bodies may have the correct number of locators, but each locator may be expanded from a "point" into an area, sometimes referred to as a "functional area". These locators are rendered effective by forces.

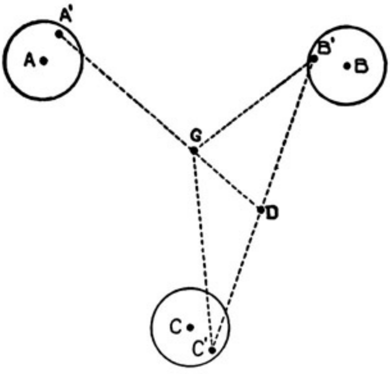

FIG. 43.

Inasmuch as ideal points do not exist, strictly speaking, no instrument gets nearer to a kinematic design than this; but more than that is implied, namely that the areas of contact are not even attempts at point contact, it being the essence of the design that the two parts shall meet with areas of contact.

Thus, Fig. 43, a three legged instrument has the base of its legs so shaped as to contact with a table over three equidistant circular areas, at least if the legs and table were perfect this would be so; and, if the centre of gravity of the instrument lay midway between the legs, then each leg would take an equal load, and the centre of pressure of each leg would lie at its geometric centre, A, B, and C.

Actually, owing to imperfections, it is uncertain what proportion of the pressure each element of a leg's surface is sustaining, and therefore where, within the surface, the centre of pressure lies; thus the three legs may have their centres of pressure at A', B', and C'. This uncertainty is great just in proportion to the areas involved, and it becomes zero if the legs shrink to the three points A, B, and C, from which the functional areas may be said to have grown. A similar uncertainty evidently exists as to the total load carried by each leg; thus, in the figure, if G be the plan of the instrument's centre of gravity, and if the total weight be W, the leg A will sustain;

$$\frac{GD}{A_1D}W,$$

and similarly for the other legs.

These uncertainties, however, are not likely to lead to any great change in stresses and do not lead to incomplete elements; for the areas are a minimum in number, and uncertainty as to the exact position of each locator is not sufficiently great to remove the effecting force on each, such as would occur if the locators were all in one straight line. Assuming a reasonable design, in which the area presented by each leg is small compared to the distance between them, considerably greater carrying capacity has been secured at no serious loss of predictability, either as regards forces or geometrical properties, and the completion of no element is jeopardized.

2. A second retreat from pure kinematic design occurs when the areas of contact are still a minimum in number; but, instead of each area presenting redundant locators leading to redundant constraint, as in the above example,

each area is in itself overconstrained, being rendered effective by an opposing locator or locators.

Thus, in the previous example, each leg might be secured to its supporting stand by bolts or studs as shown in Fig. 44. Here the locator—the surface of the stand or table—is rendered effective by another locator, namely the under face of the nut. A nut or bolt regarded as a locator has this peculiarity in common with a force, that its action is independent of any dimensional accuracy; since, in the act of screwing it down, its position is automatically cared for; in fact, a nut used in conjunction with a spring washer may be regarded as a very stiff force; but the stresses far exceed those usually associated with effecting forces, and in practice are subject to little control and much uncertainty.

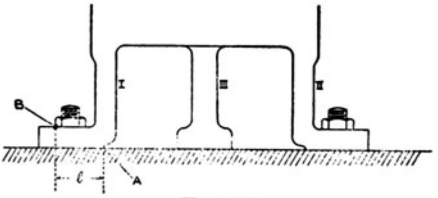

Fig. 44.

The stresses caused by overconstraining the minimum number of areas of location have this peculiarity; in the main they are contained within the area itself, and hence, unless friction is a consideration (which is not the case when the constraint is intended to remove all motion), these stresses are easily cared for and do little harm. Thus in Fig. 44 suppose that by some mischance leg *I* bears exclusively at *A*, and the nut bears only at *B*; this is the worst possible case; but if the nut is exerting a comparatively large force *F* on the leg the torque transmitted to the instrument frame is only,

$$Fl;$$

where l can easily be kept reasonably small by not providing an excessive area to the base of the leg itself, and is of no great consequence. It is true that if the instrument is carelessly bolted down a far worse case may arise. Suppose, Fig. 45, that the leg *I* is slightly warped, and is bolted home before the nut on leg *II* has been run down; then on tightening nut *II* a torque of *FL* is generated which will probably snap the first leg or distort the instrument; hence the importance of going round the nuts by degrees. This danger can be avoided by deliberately making the sole of each leg a shade short at the heel.

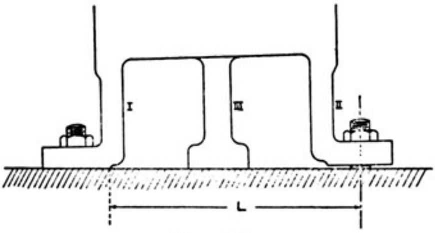

Fig. 45.

When constraining a moving part the results of departing from a kinematic design are somewhat different.

Consider the constraint of a sliding rod or cylinder; a completely kinematic arrangement would consist of two *V* supports, as shown in Fig. 39(*A*), one near each end of the rod; each limb of the *V*'s being rounded where they touch the rod, in order to give "point" and not "line" contact. There is no object in departing from this design unless the rod is unusually heavy (as the usual run of instruments go), or for some reason is subject to heavy forces, such as the lateral forces arising from fast rotation, etc.; in this case the kinematic design is unsuitable for several reasons; first because the effecting force would have to be relatively large, leading to appreciable wear and friction at the point of contacts; and secondly because point contacts are the worst possible form of bearings for retaining any lubricant. Here ball, roller, or plain journal bearings, are clearly indicated, and, provided the minimum number of areas are employed as locators, the overconstraint in each area does not lead to heavy stresses along the shaft. When the requirements as to accuracy of position are not very exacting, the ungeometric journal bearings may result in less forces and friction altogether than a geometric design, especially in the presence of occasional large lateral forces (due perhaps to severe vibration or rotation, etc.). In such a case the bearings are sufficiently easy to prevent any tendency to bind (the resulting play, i.e., completion of an element being below the significant level by hypothesis), and no forces are placed on the shaft by the bearings themselves. Any kinematic design with its effecting forces tends to have the latter as large as any expected disturbing force, and it is continuously acting whether the disturbance is present or not. On the other hand, if great accuracy is required, the element is necessarily liable to be incomplete with an overconstrained design; and, if this is prevented, great local stresses may easily occur; moreover such elements become incomplete with wear.

3. A third departure from kinematic design consists in providing redundant areas of location all rendered effective by forces. Assuming the parts small and light, and the forces also small, the stresses may still be insignificant; but in general the element in question will be incomplete, and to an extent not always easy to predict or control. Thus a four legged instrument is apt to wobble uncertainly between two diagonally opposite legs, whilst an extreme instance of this is an instrument without legs, presenting a nominally flat surface to the table. Moreover, the geometrical uncertainty bears no relation whatever to the extent of contact area secured.

4. Finally the areas of location besides being redundant may be overconstrained. This represents a complete antithesis to kinematic design in every possible way. In the case of moving parts so constrained, either incomplete elements, or excessive forces, or both, are almost inevitable; moreover, the main stresses are no longer confined to the immediate neighborhood of the locators but are distributed throughout the body of the instrument. Thus, in the case of a four legged instrument with all legs bolted down, if the

legs are at the corners of a square whose diagonal is L, and the force between any one leg and its nut be F, then the body of the instrument is liable to a maximum bending movement of $\frac{FL}{2}$. The same warping action occurs in the case of a slide constrained by guides in two areas widely separated.

DEFINITION: SEMIKINEMATIC ELEMENT

Broadly speaking a design can be looked at in two ways: in detail and as a whole. In the latter, or macroscopic view, no notice is taken of detail within a small area, only the main disposition of the parts are perceived; thus the areas of location are observed but no detail within those areas. It is immaterial in this view whether locators are "points" or areas, or whether the effecting agents are also locators or forces.

If a nonkinematic design appears to be kinematic when viewed macroscopically, it will be called a semikinematic design. Thus, if in a semikinematic design, each locator is reduced to a point, and effected by a force, the design becomes fully kinematic.

Semikinematic designs fall into two groups according to whether each area is overconstrained or merely redundantly constrained. If the latter, elements are complete irrespective of dimensional accuracy of manufacture, forces are very little different from a fully kinematic design, and the properties of the instrument are very reasonably predictable. The loss in prediction of forces and geometrical conditions generally being proportionate to the size of the locators.

An overconstrained semikinematic element will not in general be complete unless constraint is intended to prevent all motion between the links; but in any case the parts can be so designed that stresses set up are mainly concentrated in and about the areas of location themselves, with the exception of forces resulting from friction of the moving parts. Moreover such secondary forces as are not so localized are usually proportional to the size of the locating areas. Here again, a macroscopic examination of such a design fails to distinguish it from a kinematic design, but a microscopic view does so; the design is semikinematic.

A nonkinematic design is obvious when considered as a whole, in that the areas of location are redundant. If not overconstrained great incompletion of elements is liable to occur; whilst if each area is overconstrained this is commonly accompanied by great, and largely unpredictable, forces.

At the same time it must be admitted that, provided the play, consequent on a "comfortable" fit with reasonable workmanship, is below the significant level, a nonkinematic design with overconstrained locators has some very real advantages in the presence of severe disturbing causes; especially if the parts constrained are relatively heavy. For the merit of a kinematic design largely depends on the elimination of forces and hence friction, and so is particularly suitable for light parts requiring great precision of movement in the absence of severe disturbances. As soon as the effecting force is required to be great this form of design begins to lose merit; though it can usually be made to retain the quality of precision.

EXAMPLES: SEMIKINEMATIC DESIGN

Whilst admitting the limitations of fully and semikinematic design, it is often too readily supposed that nothing can be done to increase the robustness of an element without hopelessly overconstraining the links. Thus suppose, owing to its length and necessarily small diameter, it is desirable to support a rod in more than two places, it is a mistake to suppose that it must be either redundantly, or over, constrained. The Chinese are popularly supposed to have solved the problem of multiple kinematic support a thousand or so years B.C. They applied the principle to the lifting of heavy weights, and thereby secured that each porter was bearing his equal share of the load. If Fig. 46 AB represents a beam with the load W hung centrally, then if two men hold the ends A and B respectively they each support $\frac{W}{2}$ irrespective of their heights or the states of their conscience. If the beams CD, and EF, are flexibly attached to A and B at their centres, four men at C, D, E, and F, similarly each support $\frac{W}{4}$; or, extending the subdivision once more, eight men at C_1, C_2, d_1, d_2, e_1, e_2, f_1, f_2, all bear $\frac{W}{8}$; and so on. This is a true geometric design, and the loads on each support adjust themselves irrespective of any great accuracy of the parts, or the men's heights.

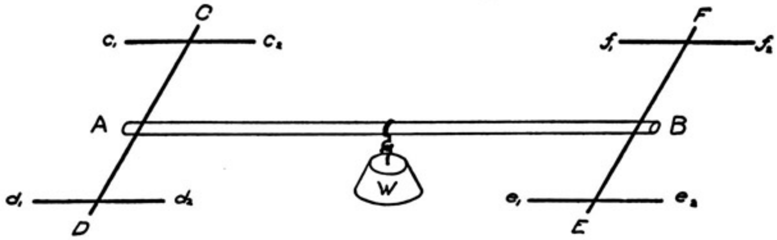

FIG. 46.

It is, of course, not necessary to increase the supports in multiples of two, and Fig 47 shows a rod supported in three and four places, each locator taking an equal load. In both cases the frames stand on the four hemispherical legs a_1, a_2, and b_1, b_2; care must be taken to constrain these legs correctly; thus in case (A), a_1 might stand in a hole, and a_2 in a slot pointing towards a_1; then b_1 should stand in a hole, and b_2 and C on planes. This arrangement can be varied in a number of ways but five horizontal constraints are required. In the case of (B), a_1 and b_1 should stand in holes, and a_2 and b_2 on planes, or vice versa. Four horizontal constraints should be provided. It will be seen that, provided the relations of lengths given in the figure are adhered to, and the supports are symmetrical about the length of the rod, all *V* supports take an equal weight. In particular this is independent of any great accuracy of dimension (the usual result of fully or semikinematic design), and independent of any particular ratio between *L* and the length of the rod; there is one particular arrangement of the supports in each case which leads to a least maximum stress in the rod, and another which results in a least maximum deflection. In practice it would be well to arrange that the legs a_1, a_2, and b_1, b_2 were in the same horizontal plane as the points of contact between the rod and the *V* supports, and also that the distances a_1, a_2, and b_1, b_2 are as small as is compatible with the necessary lateral stability; these precautions result in the least frictional obstruction to the self-alignment of the floating cradles.

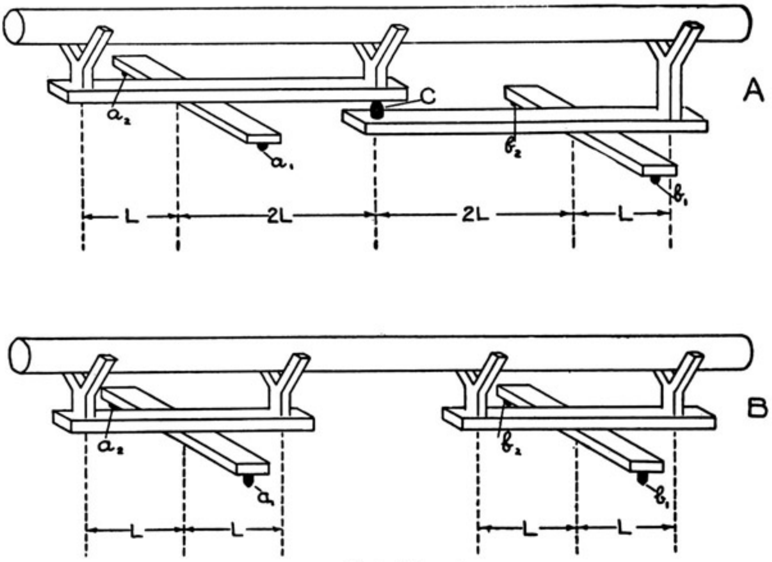

Fig. 47.

This type of support is capable of endless variation to suit particular circumstances, and is useful where a heavy weight has to be supported with the minimum risk of distortion. It was suggested at one time that the principal mirror of very large reflecting telescopes might be supported in this manner, but I am unable to state whether this was ever done.

An instance in which a resistant semikinematic element was called for occurred in connection with an instrument weighing some 70 or 80 pounds. This instrument, which was of the order of 18 inches cube, had to be mounted on an adjustable pedestal in a ship. The conditions were difficult, as the instrument was one of the highest precision, and it was vital that it should not be distorted in the process of being secured to its pedestal. On the other hand, the slightest movement between the pedestal and the instrument would have rendered it useless. Finally the instrument was subject to rolling, pitching, yawing of the ship in a seaway, and the rather severe vibration caused by the ship's engines and screws. The circumstances were such that some form of semikinematic constraint was essential if the heavy stresses were not to be introduced into the body of the instrument due to clamping, and, owing to the remaining conditions, it did not seem wise to trust to three areas of contact alone; consequently it was decided to bolt the instrument to its stand in four places, and the problem consisted in doing this without introducing redundant areas of contact. The method adopted is shown diagrammatically in Fig. 48. The four studs on the instrument fit into the oversized holes and slots similarly lettered on the stand; the instrument is located vertically by the three locators v_1, v_2, v_3, and in one direction horizontally by the two locators h_1, h_2. The last locator, horizontally and at right angles to the first, is not provided but this was of no consequence, as the stand was adjustable on the base of the pedestal.

The method of clamping is to screw up the nuts on the H studs, until the reverse face of H_1 bears on h_1; and similarly with H_2 and h_2—the V nuts are then lightly screwed to ensure that V_1, v_1 and V_2, v_2 remain in contact; next the H nuts are tightened, and finally the V nuts.

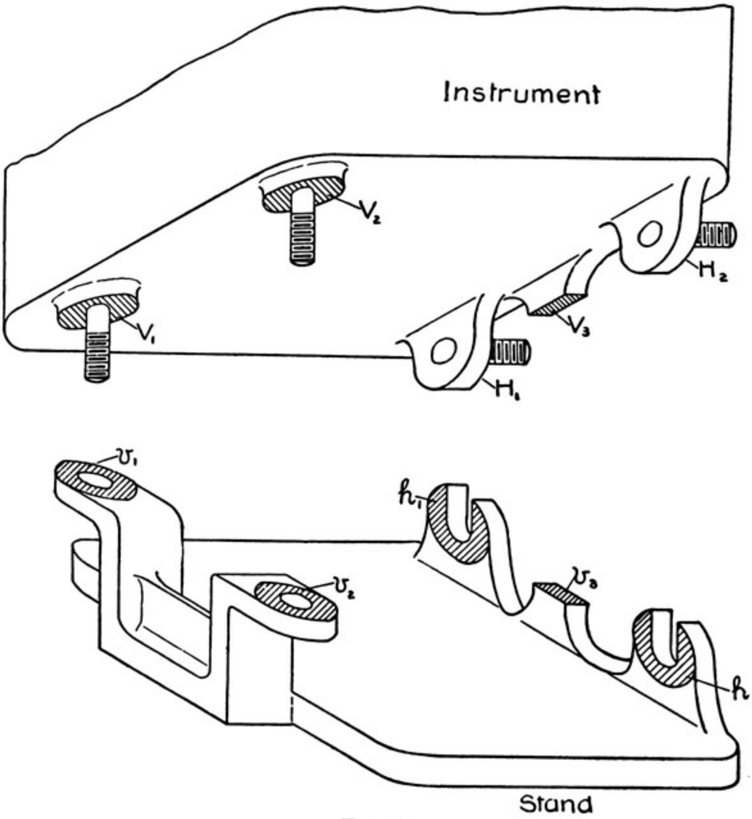

FIG. 48.

Five locators are used out of a possible six, the last location being maintained by friction between the bolted surfaces; of these five locators, only four are bolted in this case; but with a suitable design six locators, all bolted, can be used without departing from a semikinematic design. In practice however, this might prove dangerous, unless the theory of the support is thoroughly understood by the operator responsible for the actual clamping. Thus, in the present case, if the *V* nuts are tightened before the *H* and *h* surfaces are in contact, then, when the *H* nuts are tightened, heavy stresses in the instrument and stand will result; with six bolted locators this mistake would be very easy.

FURTHER LIMITATIONS OF KINEMATIC DESIGN

Although a kinematically designed moving part maintains its element complete in the presence of dimensional errors, yet it is peculiarly sensitive to surface faults by reason of the fact that the surface on which a given locator slides only supports the latter at one "point", which consequently gets the full benefit of any irregularity. This limitation is not so great as might appear, although it is sometimes advisable to expand the "points" into areas or very slight curves (giving a flat "point"), for the reason that in most cases these surfaces can be made plane or cylindrical with a very high degree of surface, as distinct from dimensional, accuracy—and at a comparatively low cost. In consequence the errors to be feared from this source are usually considerably less than those due to an incomplete element resulting from a nonkinematic arrangement; the thread of a precision screw is an exception to this rule, and is one reason why the nut is arranged to cover a considerable length of screw. Even in this case it is really periodic errors resulting from incorrect geometric form, rather than inferior surfaces, which are feared.

Nevertheless if short period or systematic errors are to be avoided with kinematic design first-class surfaces are frequently required, and partly for this reason planes, cylinders and spheres, and other simple shapes are chiefly used. The sphere is not altogether an easy form to manufacture accurately, but, in small sizes, makers of ball bearings have specialized in this problem and sell balls very cheaply to remarkably fine limits.

In order to secure precision not only must disturbances be small and subject to restoring agencies, but, in the case of instruments whose accuracy is to be increased by taking the mean of repeated observations, the short period errors must be unbiased; it has been seen, in Part I, that this implies similar restoring moments for deviations of either sign; here fully—and semikinematic elements, in which the locators are rendered effective by forces, are a grave disadvantage for disturbances in one direction are countered by locators and in the other by yielding forces. For this reason, such designs are only effective where likely disturbances are below the significant level. Owing to the much lighter construction of kinematic elements this condition is fairly easily satisfied, except in the case of very severe vibration, or other large disturbing causes; for the same reason less disturbance will usually occur through the flexure of the parts themselves.

CHAPTER IX

STIFFNESS, ISOLATION, AND PROTECTION OF ELEMENTS

ELASTICITY OF MATERIAL

The elasticity of materials, in so far as this concerns the behavior of locators, has been sufficiently considered; but very little has been said regarding the stiffness of members in themselves; and by this I mean elements entirely composed of one continuous piece of material, or of pieces so secured or bolted together as to behave as one piece.

It has already been stated in an earlier chapter that, whereas the parts of a single member may form an element, in practice this is not often necessary, simply because such elements are not usually the cause of serious error by disturbance. This is true but, although such elements can generally be made adequately stiff, it is a fact that this precaution is sometimes neglected with unfortunate results; and, moreover, very little information with respect to the vibration of small members is available.

AN EXAMPLE

Consider the design of a pedestal for an instrument weighing about 150 lbs. to be mounted 5 feet above the ground. One such instrument in which I was interested had to be so mounted in the open; it was thus exposed to the full force of any wind. In addition the instrument had to be trained in azimuth, and locked in one of a number of positions with the minimum possible loss of time; this was to be effected by substantially the means already shown in Fig. 42(*C*); obviously this clamping device, used in a hurry, would be liable to check the rotation of the instrument violently, and to set up all sorts of vibration. Since the instrument had to be significantly free from vibration within less than a second of clamping, the stiffness of the support became important.

In cases where a permanently located stand or pedestal is required for an instrument, a convenient and cheap arrangement is to use an iron or steel pipe, cut to the required length, with either a standard flange at each end, or better still two castings, one to take the instrument and the other to be secured to the ground (e.g., bolted down to a concrete platform). For the instrument under consideration the free length of such a pipe would be about 4 feet; and I will suppose that its external and internal diameters be chosen to be,

$D=5''$ and $d=4''.5$ respectively.

Now the area of this instrument exposed to the wind is about two square feet, neglecting the pedestal, and a moderate gale will produce a wind pressure of about 5 lbs. per square foot. Hence the instrument might well be sustaining a horizontal force of 10 lbs.; and, since it is used for measuring angles, the angular deflection of the pedestal due to this force should be calculated.

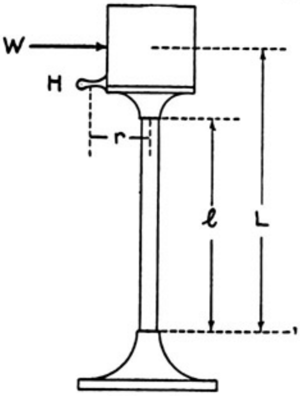

FIG. 49.

Referring to Fig. 49, it is assumed no deflection that occurs in the flanges or instrument itself, and is therefore confined to the length l.

$$L=54 \text{ inches.}$$
$$l=48 \text{ inches.}$$
$$W=10 \text{ lbs.}$$

The formula for the angular deflection of such a cantilever, expressed in radians, is:

$$i=\frac{W}{EI}\left(Ll-\frac{l^2}{2}\right).$$

E, the modulus of elasticity, $=30\times 10^6$ lbs./ins.^{2} for mild steel. I, the 2nd moment of the cross section of the tube about a diameter,

$$=\frac{\pi}{64}(D^4-d^4)$$

$$=\frac{625-410}{20.2}$$

$$=10.6.$$

Hence

$$i=\frac{10}{30\times 10^6\times 10.6}\left(54\times 48-\frac{48^2}{2}\right)$$

$$=45\times 10^{-6}\text{ radians}$$

$$=9\text{ seconds, approximately.}$$

Whether such a deflection, and a biased one at that, can be tolerated depends entirely on the nature of the observation, and the accuracy required; but the point to notice is that a horizontal force of only 10 pounds has caused the instrument to cant out of the vertical by an angle several times greater than would be tolerated in a reasonably accurate survey; whilst a heavy gale might well produce double this deflection. Moreover this small force produces a deflection in a pipe which, regarded as a column, would easily bear a load of several tons; and, even as a cantilever, 10 pounds is less than 1% of its safe load.

The nature of the observation required the instrument to be traversed and clamped in the least possible time; and it is for consideration whether the observer might not easily produce a second horizontal force of at least the same order, by throwing his weight suddenly onto the training mechanism at the head of the column. A glance at the formulae for I and i shows that the resistance of the column to deflection varies as the first power of I, and that this can be increased far more easily by increasing the diameter of the pipe, than by merely thickening its wall. In fact the deflection would be barely reduced to a third of its former value if a column of solid 5 inch steel bar were substituted for the pipe. This gives some idea of the extraordinary disproportion between the shape and size of a member designed solely for strength, and one designed to be sufficiently stiff for an accurate observation.

DAMPING

The stiffness of an elastic member is important in another way, for not only should likely disturbances fail to produce significant errors, but, when a more or less significant disturbance has occurred, it should very quickly be damped out. If the member were perfectly elastic, and no external damping existed, a disturbance would result in a permanent vibration. Actually air resistance, and, to a greater extent, the internal friction of the material composing the member does dissipate the stored energy, and, in the absence of further disturbances, cuts short the vibration. In a given case it is not easy to foretell what degree of internal damping a member will possess. In the case of a well designed tuning fork damping is extraordinarily little, but becomes far more pronounced in cast members of irregular shape. Experience shows that a stiff member, that is, one with a high period, damps noticeably faster than a similar but less stiff part; and this is in line with common sense theory.

Some rough idea of the sort of time taken for a vibration to damp out in a member of simple form may be obtained by calculating its period roughly, assuming of course its actual loading. Since any member is capable of vibrating in more than one way, care must be taken to choose the vibration with the longest period. As a rough guide I assume that for a vibration of the form considered to be sensibly dead beat, in an iron casting of approximately uniform section, it should have a period of about 200 complete vibrations per second at least (not far below middle *c*). This is more a nominal figure for design purposes than a serious statement of fact, for the actual time taken depends greatly on the precise form, size, and method of support, etc.; but it is a useful, though very rough guide; a steel tube might be expected to have a smaller decay. Incidentally the time required for decay is not so sensitive to the amplitude of the initial disturbance as might be supposed, for very severe vibrations rapidly decay. Equally, the moment of complete extinction is fairly definite, where the member is sufficiently stiff not to be in a continuous state of disturbance from minor causes; for the internal friction entirely prevents the action of restoring moments below some finite value.

The danger of more or less perpetual vibration of significant magnitude is one of the bugbears of designers of accurate instruments, and research leading to some practical data on this subject for various types of members is urgently required.

EXAMPLE

Returning to the instrument supported as shown in Fig. 49 let us find the period of transverse vibration of the column, regarded as an elastic cantilever, built in to the lower casting, and loaded with 150 pounds at its free end. The time, T, of a complete vibration is, of course, given by the expression,

$$\frac{2\pi}{\sqrt{G}} \cdot \sqrt{\frac{M}{f}}.$$

Where M is expressed in pounds; and f is the restoring force in pounds per unit of displacement (one foot). T is thus expressed in seconds.

From the usual formula connecting load and strain in cantilevers,

$$f=73{,}500 \text{ pounds.}$$

Hence

$$T=\frac{2\pi}{5.66}\cdot\sqrt{\frac{150}{73{,}500}}$$

$$=0.05 \text{ second (of time).}$$

Quite clearly the support is far from dead beat when disturbed, and, in an ordinarily gusty wind, would be continuously vibrating. In order to have a period of 200 vibrations per second with a wall thickness of $\frac{1}{4}$ inch as before the tube should have a diameter of about 16 inches. In practice a lesser stiffness would meet every requirement, since it is hardly possible that any tube over 12 inches would ever be sufficiently strained to render its vibrations significant with an instrument of the size and weight considered; but it is interesting to notice that, if the instrument be required to read vertical angles to one second in a high wind, no smaller tube than one of about 12 inches diameter would suffice to support it.

The instrument is rotated in azimuth by a handle, H in Fig. 49, and, assuming that the observer presses on the handle just before releasing the clamp (by a finger grip on the handle), he may well exert a 20 pound pressure tending to twist the tube with a torque equal to $20r$ pounds. Assuming r to be 15 inches the twisting moment on the tube is 300 inch/lbs., and the resulting angular twist of the tube ($D=5''$, $d=4\frac{1}{2}$),

$$\theta=\frac{32Tl}{\pi C(D^4-d^4)} \text{ radians.}$$

Where

T = Twisting moment.
C = Modulus of transverse elasticity, about 12×10^6 lbs./ins.^{2} for mild steel.
$\theta = 0.57 \times 10^{-4}$ radians
$= 12$ seconds of angle.

Again it will be seen that the disturbance would be significant for many purposes, and it will be interesting to obtain the period of such rotational vibrations. The fundamental expression is,

$$T = \frac{2\pi}{\sqrt{g}} \cdot \sqrt{\frac{M_1}{f_1}}.$$

Where M_1 = the moment of inertia of the instrument (neglecting the mass of the pipe as before).

f_1 = the moment of the restoring forces per radian of displacement.

To evaluate M_1 the actual instrument, or complete drawings, are required; but 5,000 ins.^{2} lbs. will be assumed as a reasonable figure for an instrument of this size and weight; whilst f_1, the restoring moment, is given by

$$f_1 = \frac{c \times Io}{l};$$

where Io, the polar moment of inertia of the pipe,

$$= \frac{\pi(D^4 - d^4)}{32} = 21.2.$$

Hence,

$$f_1 = \frac{12 \times 10^6 \times 21.2}{48} = 5.3 \times 10^6 \text{ inch lbs. per radian.}$$

$$T = \frac{2\pi}{5.66} \cdot \sqrt{\frac{5{,}000}{5.3 \times 10^6}}$$

$= 0.034$ second of time.

This period is only slightly shorter than that of the transverse vibrations and the same general remarks apply.

DISCUSSION

Thus it is seen that two problems arise in connection with the stiffness of an element comprising a single member. The first relates to the error resulting from the probable magnitude of the disturbing causes; whilst the second relates to the free vibration of the element set up by any *variation* in the magnitude of a disturbing cause. Sometimes one and sometimes the other will prove the more important, depending on the nature of the observation and the probable frequency of the disturbances, etc.

The first error is often biased, and usually of too short a period, and too uncertain, for correction of any kind. Thus a wind is not equally likely to blow from any point of the compass from one moment to the next, but will stay more or less in a given quarter for the duration of an observation. On the other hand its exact direction and force will vary sufficiently over very short periods to prevent the error being treated as systematic. If the observation takes a short time, and need not be performed at any precise instant, often the designer need not cater for the more unfavorable "probable" conditions, as these can be deliberately avoided. On the other hand many instruments, especially those used in the fighting services, have to be used at some given moment, and even unusually severe disturbing causes must not produce significant errors.

The second type of error—vibration—at least reverses its sign many times a second, and, if the member is symmetrical, leading to equal restoring moments for similar departures of both signs, will be unbiased. This does not imply that the element will vibrate about the required relation, for obviously the first type of disturbing cause may or may not be superadded; but any additional error due to vibration will, in the case of symmetrical vibration, be unbiased. Thus any observation whose time of accomplishment is long compared to that of the period of vibration may well "mean out" this source of error. Photographic records are perhaps the most obvious instances of observations in which the time of accomplishment is sometimes too short for such "meaning"; it being not unusual to use exposures of from a hundredth to a thousandth of a second when photographing some changing phenomenon, and even shorter exposures are employed on occasion.

Of course the process of "meaning out" a vibrating element results in a more or less blurred impression, but if the vibrations are not too severe very often the eye, or the subsequent setting on the photographic image, can be performed with an adequate accuracy. But, as explained elsewhere, the eye can set with an accuracy greater than that with which it can distinguish detail, and a vibration so slight as not to create visible blurring may well result in a definite loss of accuracy unless the time of actual observation (photographic or human) exceeds the period of vibration.

If material were perfectly elastic and no damping were introduced, every member however stiff would be vibrating more or less noticeably when in the presence of any variable forces, however rarely they occurred or slight they might be. Actually it is very difficult to be sure what does occur, but my belief is that the facts are somewhat as follows for such members as steel pipes with end flanges, castings, etc. The member in the first place is not freely suspended at a node, but on the contrary is in contact with at least one other member, and is considerably damped at this place; secondly it is damped by its own internal friction; and to a lesser extent by the surrounding air. The first two types of damping mentioned are really a form of solid friction, and consequently there is a "threshold" disturbing cause, such that any similar cause of a smaller moment will fail to set up free vibrations in the member. Consequently most pieces of matter, such as the ink pot and ash tray on my table, are definitely not vibrating by elastic deformation during the major part of their existence, even to the smallest extent.

Of two similar members, one having a greater resistance to elastic deformation than the other, the former will have a greater internal damping force; moreover this member will have a faster natural period for a given loading. Hence the stiffer member will show a greater decay on each period, and also have more periods per second, leading to a more rapid approach to that minimum distortion below which vibration ceases. If both members are only loaded with their own weight undoubtedly the stiffer member will be more heavily loaded than the other; but, for similar sections, the internal friction will vary as the stiffness (i.e., the fourth power of a linear dimension), whilst the loading varies as the volume (the second power; for both members are supposed of equal length, being alternative designs for some given purpose). Hence in this case, too, the stiffer member will have a quicker natural period of vibration, and this vibration will cease in a smaller interval of time. So, of two similar members of different stiffness, that with the smaller resistance will be at a threefold disadvantage. First it will require a smaller disturbing cause to induce vibration. Secondly a given cause will vibrate it more significantly. Thirdly such vibrations will take longer to decay.

However quiet the conditions of observation, disturbing causes are never entirely absent, and, the smaller the moment of the causes considered, the more numerous they will be. In consequence, for any given conditions there will be some minimum stiffness for a member such that any lower value will cause it to be perpetually vibrating. This, as every one knows is the natural condition of all tall buildings in a moderate wind, and of such structures as suspension bridges, etc., but it should never be tolerated in instruments used under reasonably easy conditions; and I believe that, as a very rough rule, a period of vibration of 200 per second for small parts should be aimed at as a minimum requirement. For large members weighing many pounds this may be excessive, for the size of the threshold disturbance depends partly on the absolute size of the member in question.

In the case of instruments on a moving vehicle or ship, or exposed to strong wind or other violent causes of disturbance, it is not always possible to ensure that vibrations will not be started with any reasonable stiffness; and the only course is to arrange that the frequent causes shall be catered for, in the sense that the resulting disturbances shall be insignificant; whilst the larger, but still not improbable causes, should either lead to practically dead beat disturbances, or these should still be insignificant. Which alternative is adopted depends on the nature of the observation and also on which alternative leads to the lighter and cheaper design. In the case of the support for the instrument considered above, it proved far easier to make sure that disturbances should be insignificant than that the vibrations would be dead beat. In fact in any moderate wind this instrument would be vibrating through a minute angle practically continuously even with a 12 inch pipe to support it.

The foregoing considerations apply to some extent to elements not only composed of single members, but of every other description. However in elements of other types the damping through external causes (such as friction between two parts) is usually sufficient to make a departure practically dead beat whatever the element's free period. Consequently it is in elements comprised of single members that vibration is usually most to be feared in instruments; and in nine cases out of ten the main base or frame is the element most liable to this fault, just because it so often is the largest single member in the instrument and is most heavily loaded, and tends to be, and look, surprisingly heavy if made adequately stiff. The choice of a suitable section can do much for its absolute weight and appearance. In this connection it must be remembered that a built up "lattice" structure, so beloved of structural engineers, gives excellent strength for weight but is apt to be very poor as regards stiffness, because each subordinate member, especially tension members, may vibrate on their own (being loaded through their own weight). Hence subordinate members must be stiff enough to ensure that no "subcomponent vibration" occurs. The base of the Cambridge Measuring Microscope, illustrated in Fig. 67, is an excellent example of a well designed casting which acts as the backbone of a really accurate instrument.

ISOLATION OF ELEMENTS

Where an element cannot easily be made sufficiently insensitive or stiff in the presence of a disturbing cause, the former can frequently be isolated or shielded from its effects. Thus: sensitive moving parts are shielded from draughts; other instruments may have their temperature held approximately constant by a thermostat; small electric instruments are sometimes shielded from magnetic fields by being enclosed in a thick casing of soft iron.

In the National Physical Laboratory (England) a number of devices [1] are used to shield the more accurate measuring instruments from the effects of various disturbing causes. For instance, the rooms used for the comparison of principal standards are all entirely surrounded by other rooms or passages, outside walls being eliminated, and lit by a double glazed ceiling, above which is a roof containing double glazed northern lights; this assists in the elimination of temperature variation, which is further reduced by controlling these rooms thermostatically. Beyond this the instruments are further screened from such things as the observer's body and small artificial lights. The latter preferably take the form of collimated light sources directed at the apparatus from a distance. In the case of some instruments, such as the most delicate balances, arrangements are made for distant control and observation, in order to avoid the presence of the observer.

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[1] Described in the *Dictionary of Applied Physics*, Vol. III, "Metrology" by J. E. Sears.

Much the same sort of precautions are adopted when engraving circles of the highest accuracy for angle measuring instruments. The room in which the engraving is actually proceeding is unoccupied, and the progress of the work is controlled by observing it through telescopes let into the adjoining wall. This avoids the risk of draughts, changes of temperature, and vibration.

Screening with a view to avoiding temperature effects may take various forms depending on the desired result. In the first place the instrument may be surrounded by brightly polished surfaces to reflect radiant heat; it may also be surrounded by some nonconducting material, such as asbestos, which transmits heat very slowly. Finally the instrument may be enclosed in a thick walled box of good conducting material; the object of this is to secure that the instrument shall receive such heat changes, as cannot be eliminated, uniformly on all sides, to prevent local distortion; the box having an approximately uniform temperature all round in virtue of its conducting properties. These methods of temperature shielding are sometimes used concurrently, the outside lining being of brightly polished copper which has an excellent thermal transmission, whilst the inner lining may be nonconducting. Alternate layers of conducting and insulating material are used on occasion. The same precautions are needed to ensure an absence of draughts, for any local change of temperature within any enclosure at once leads to connection currents. Alternatively in some cases it is practicable to evacuate the enclosure and use the instrument in a vacuum.

Instruments are shielded from movement by a number of means depending on exactly what is to be achieved. Complete immobility with respect to the mean geometric figure of the earth [1] is usually desired, but this is often out of the question, and a compromise must be sought. Thus instruments for use in a ship or aircraft are necessarily subject to accelerations, and all that can be done is roughly to eliminate angular accelerations by means of suitably damped gymbol rings, and to damp out the incidental vibrations by some sort of shock absorbers, as in the case of gyrocompasses.

In the case of large cameras for use in aircraft, translational vibration is of less importance than angular excursions; for the former alone do not significantly alter the position of the image of a distant object on the photographic plate. Consequently such cameras are frequently attached to the aëroplane by attachments in a horizontal plane containing the centre of gravity of the instrument, and symmetrically disposed about the latter.[2] If this be done translational vibrations do not tend to rotate the camera. These attachments may consist of springs or rubber to avoid the yawing effect of the vibrations; in one design the camera is mounted on tennis balls.

On land one of the best supports for an instrument, such as a galvanometer, is said to consist of a pile of alternate layers of thick felt and paving stones, the alternate cushions of felt enabling the massive stones to remain immobile in space in the presence of rapid vibrations. If my memory is not at fault this device is due to Professor C. V. Boys, but I cannot find the reference. Naturally, to avoid surface tremors, such a column should rest on the bottom of a pit, and not make contact on its sides.

In many cases, where the effect of some unwanted variable has to be eliminated to a high degree of accuracy, it is simpler and more effective to obtain the required isolation by using a number of independent means, rather than by trying to perfect one method beyond its comfortable limit. An example of this was seen in the simultaneous adoption of various types of temperature lagging, in conjunction with thermostatic control. Another example is the adoption of the pendulum for clocks, whose period is approximately independent of the amplitude of the former's excursions, combined with precautions for maintaining an unvarying amplitude of swing. Again, at least one make of astronomical clock secures a constant pendulum length by making the latter of invar, and adopting thermostatic control. Not content with the latter, which greatly reduces the risk of air currents in the pendulum box, the box itself is evacuated.

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[1] Not "complete immobility with respect to the earth," because crust of the earth often suffers tremors, which it is sometimes the object of the shielding to avoid.

[2] *Airplane Photography* by Major H. E. Ives of the United States Army.

Discrimination between Variables

A special and very frequent case arises when the unwanted disturbance is of the same nature as the phenomenon which the instrument is intended to observe; here the problem consists in discriminating between the two phenomena in some way. Thus photographic plates and films are essentially intended to be sensitive to light, and are shielded from all light which does not enter the lens on controlled occasions. In some cases the discrimination between wanted and unwanted variables of the same type presents considerable difficulty; for some invariable difference between the two must be found which renders them capable of separation. Thus, for various reasons, it is sometimes desirable to know the absolute vertical at sea, in connection with other researches; the problem is to discriminate between the forces due to gravity and the accelerations of the ship due to rolling, pitching, turning, rising and falling, alteration of speed, and any combination of these. These forces are precisely the same kind, and much misplaced ingenuity has been expended in trying to design such devices as pendulums which shall be acted on by the force of gravity alone. There is, however, one way in which the latter force is quite unlike all the others mentioned; namely in its duration. No ship has a greater period than a minute as regards rolling and pitching, and usually it is decidedly less. Accelerations due to change of speed may last considerably longer, but then they will be slight, and even they don't last for ever. Accelerations due to turning can well last more than a minute and may be relatively severe, whilst rise and fall, depending as it does on the passage of a wave with respect to the ship, will not usually last more than a minute even in the case of long oceanic rollers. Duration is about the only invariable difference between the wanted force of gravity and unwanted forces; and all successful methods of indicating the direction of the former by direct means consist, essentially, in making a pendulum of such a long period that the temporary forces have little time to act; the gyroscope when used as a vertical finder is essentially an extremely long period pendulum.

It has been seen earlier that the Wimperis accelerometer distinguishes between certain wanted and unwanted forces, due to acceleration, by taking advantage of the fact that they act in different directions.

Another example known to every amateur photographer arises when a photograph is required of certain stationary objects temporarily obstructed by moving parts. The simplest case is when it is desired to photograph a building which is continually obstructed by moving traffic; here again duration is the discriminating factor, and, by shutting down the aperture of the diaphragm almost to pinhole size, and taking an exposure of many minutes, an excellent photograph of the building will result without any trace of the temporary obstructions.

A more interesting example is given in Francis Galton's famous *Enquiries into Human Faculty;* here the author attempts to record the facial characteristics common to Englishmen by photographing a number full face, reducing the photographs to the same size as regards some one dimension, and using all the negatives successively with the same positive in order to produce a composite picture. The resulting "typical" Englishman is a somewhat full faced idiot to judge by appearances; though this may only indicate that our intelligence, or at least its expression, varies so markedly from one individual to another that its algebraic sum is more or less zero. This at any rate is an interesting attempt to sort out standard from variable factors, and indicate which factors belong to each type; there should be scope for this method of discrimination in other fields.

A good example of mechanical compensation involving discrimination is shown in J. W. Barnes' compensated altimeter [1] mentioned in Stewart's excellent book on *Aircraft Instruments.*[2]

An altimeter is essentially an aneroid barometer having a large range below ordinary atmospheric pressure, with its dial marked directly in feet, and used for determining an aircraft's height above the land. Such an instrument consists of two parts; the first a thin walled evacuated box or capsule, sometimes called a diaphragm, whose sides are distorted in varying degree depending on the external air pressure; while the second part consists of linkwork, devised to gear up the rather small movement so produced, not more than $\frac{1}{16}$ inch, and thus provide an open scale. Figure 50 shows a section

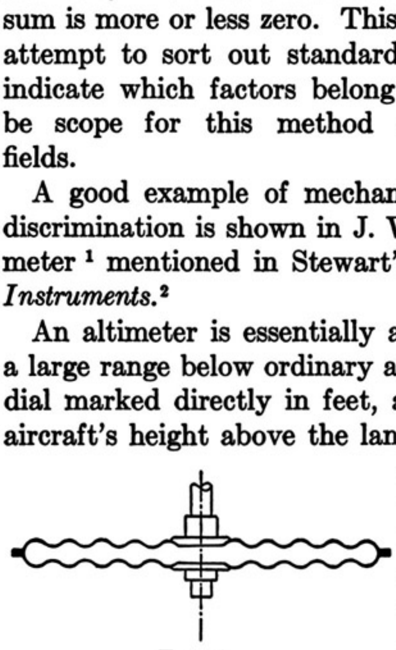

FIG. 50.

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[1] Patent specification No. 14,844/22.

[2] *Aircraft Instruments.* J. C. Stewart, Chapman & Hall.

through a diaphragm which usually has a diameter of about 2 inches in altimeters as used in aëroplanes.[1]

It was found that early types of altimeters suffered from considerable lag when ascending or descending rapidly, even amounting to a lag of 500 feet. This was a possible source of danger and has been practically eliminated in a number of ways. First it was found that the solder used to close the diaphragms was partaking of the flexure of the walls, and, being very imperfectly elastic, introduced a lag with a slow creeping recovery; the remedy consisted in using as little solder as possible, and in such positions and forms that it was placed under the minimum stress; this rendered the diaphragm inherently less sensitive to the *rate* of the pressure change. For a similar reason the total strain is kept as low as consistent with satisfactory recording, and improved materials are used. J. W. Barnes' patent aims at still further reducing this defect by an ingenious device. Figure 51, which is a simplified copy of the patent specification drawing, illustrates the principle, and the following explanation is quoted from Stewart [2] whose description is less involved than that of the patent specification.

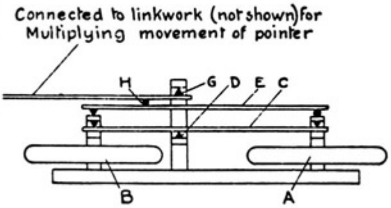
Connected to linkwork (not shown) for
Multiplying movement of pointer
H
G
D
E
C
B
A

FIG. 51.

> Two evacuated diaphragms *A* and *B* are coupled by the spring *C*, having its fulcrum at *D*, in such a way that the relative pulls of the spring upon the diaphragms are in a ratio of about 5 : 3. The diaphragm *B* is pulled out farther against the pressure of the atmosphere than *A*. The movements of both diaphragms are added in unequal ratios by the lever *E*, and the movement of the pointer is made proportional to the movement of the point *H* on that lever relative to the fixed point *G*.

[1] *Aircraft Instruments*. J. C. Stewart.

[2] *Ibid.*, p. 35.

The operation is as follows:

The whole function of *A* is to control the spring pull on *B*. When the instrument is submitted to reduced atmospheric pressure the diaphragm *A* releases the spring pull on *B* nearly as fast as the air pressure diminishes. *B* consequently moves very little in response to the difference of pressure.

It can, as a matter of fact, easily be arranged that *B* moves negatively, i.e. collapses under a reduction of air pressure. As arranged, however, the movement of *B* is positive, but is only about $\frac{1}{25}$ of *A*. *B* has little or no lag since its movement is small. *A* may have a large amount of lag, but the change of the lag in *A* causes such alteration of the spring pull on *B* that *B* moves at constant pressure. There must, therefore, be a point on the lever *E* about which it will rock for lag changes, and another point either on the lever or on a prolongation of it about which it will rock for pressure changes. The multiplying movement of the pointer is applied to the lever at the first point, *H*, the other point is somewhere to the left of *B*, so the pointer shows pressure differences without any lag.

The problem facing the inventor was to record movements of the diaphragm due to variation in air pressure, but to render the recording mechanism insensitive to precisely similar movements due to lag. He first coupled two diaphragms together in such a way that one suffered practically no movement, and hence was free from lag; this at once opened the way to discrimination, since both diaphragms exerted a force when the air pressure changed, but only one in other circumstances. Use was then made of this difference as described above.

THE MOMENT OF A DISTURBING CAUSE

As every one knows, the devices for rendering disturbing causes harmless are legion, and the object of grouping some of these under a few headings is to bring into prominence useful general ideas, rather than to present exhaustive classifications.

We have seen that some causes of disturbance can be countered by making the elements sufficiently stiff to take the buffetting without flinching unduly; in other cases the causes can be brought to a standstill before reaching the element, as in the case of thermal screens, shock absorbers, etc. Yet again disturbing causes may be prevented from ever coming into existence, e.g., holding a temperature constant by a thermostat, and balancing rotating masses to avoid the creation of vibration.

On occasion it is neither possible to screen the disturbing cause from an element nor yet stiffen the latter to any great extent, but another principle may be invoked; the moment of the disturbing cause with respect to the element may be reduced to smaller dimensions.

Consider an oscillating platform, such as the deck of a ship with the sea on its beam, and suppose that at a given instant the platform be accelerating in its own plane to an extent, a. Then the resulting force on any link of an instrument on that platform is,

$$Ma,$$

where M is the mass of the link.

a is the disturbing cause, and Ma is its moment about the link. By hypothesis nothing can be done to modify the cause a, but its moment Ma can be reduced in proportion to a reduction in the weight of the link. This may be accomplished by accepting a less strong, or stiff, link; or by a better design; or by a change in material (e.g., substituting an aluminum alloy in the place of iron).

Again it may be that a given link cannot have its temperature controlled, but the use of invar in place of some other steel greatly reduces the moment of the disturbing cause.

Another instance was given above in which a mass (a camera in fact), subject to linear accelerations, was saved from angular disturbance by supporting it symmetrically about its c.g. A linear acceleration a is the disturbing cause and the moment of this cause with respect to the mass is,

$$M.a.l;$$

so far, that is, as angular accelerations are concerned. This moment was reduced to zero by making the distance between the line of force, as applied through the supports, pass through the c.g. of the mass; thus ensuring that,

$$l=0.$$

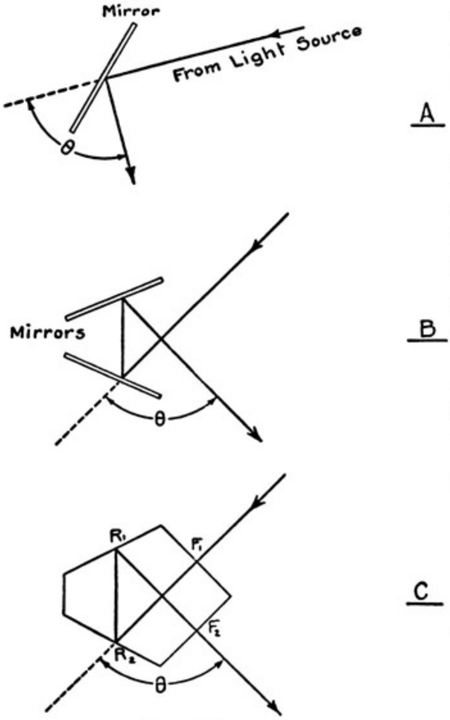

FIG. 52.

Suppose again that it is desired to reflect a narrow beam of light from some distant and stationary source through some definite angle θ; this may be done by a mirror as in Fig. 52(A). If any cause of disturbance causes the mirror to rotate round an axis normal to the paper by some angle δ the reflected angle becomes,

$$\theta \pm 2\delta;$$

the error ϵ being

$$\pm 2\delta.$$

Alternatively the beam may be reflected through the required angle θ by means of a double reflection as shown in Fig. 52(B). And the peculiarity of such a system is that θ remains unaltered if the two mirrors *as a whole* are rotated through an angle in the plane of the paper. Hence, provided that the mirrors are securely held with respect to each other, the system becomes entirely insensitive to disturbances in the plane of the paper, and is significantly so as regards small angular disturbances about any other axis. I am assuming that if, due to disturbances, the beam does not lie wholly in the plane of the paper, θ will be measured as the angle of reflection projected onto this plane. If, on the other hand, the angle between the mirrors is disturbed in any way by an angle δ the magnitude of ϵ will be 2δ as before.

Figure 52(C) represents the most secure method known for holding the two mirrors at a constant angle, for they consist of two silvered reflecting faces, R_1 and R_2, of one block of glass. Thus, short of actual fracture, or distortion due to rapid and unequal heating, this form of double reflecting prism is insensitive to almost every disturbance. This form of prism is often used, particularly in range finders, where $\theta = 90°$, and is then called an "optical square" or "pentagonal". It is not quite true to state that such a prism has all the virtues of the two mirrors (case B), for, if the beam does not cross the faces F_1 and F_2 normally, it will suffer unequal refraction at the two and θ will be affected. This source of error is not significant for small deviations, such as might occur through vibration or slight distortion in a well designed instrument.

In this last device it is seen that errors have been prevented by two separate methods. First the moment of the disturbing cause is reduced to zero, provided that two links (the two reflecting mirrors or surfaces) can be related in an invariable manner. Disturbances of this last element are eliminated, not by reducing the moment of any possible disturbing cause, but by designing an element of immense stiffness, which can hardly fail to stay put throughout its entire life.

COMPENSATION

Provided a given part is not required to be sensitive to phenomena of the same type, if it cannot be made insensitive, it can often be automatically compensated for unwanted disturbances, by being combined with some other part whose similar reactions are of the opposite sign, or can be opposed; thus, the pendulum of a clock can be compensated for variations in length due to temperature changes, by building into it a tube partly filled with mercury; a substance both heavy and subject to a large temperature change of volume. As the pendulum lengthens the mercury rises in its tube by expansion, and, given a correct proportion, the centre of gravity of the two with respect to the support, remains unaltered. Similarly the inertia of the balance wheel in the case of watches is often compensated for temperature by building it in the form shown in Fig. 53. The strips of metal *AA* added to each limb have a lower coefficient of expansion than the metal of the latter, and hence, as the wheel expands as a whole, the limbs curl towards the centre at their free ends *BB* and provide compensation.

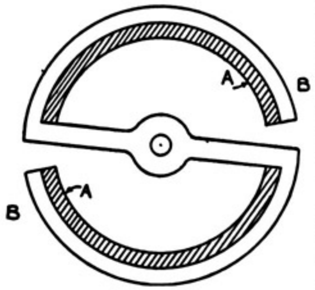

FIG. 53.

Where some part of an instrument remains unavoidably sensitive to an unwanted cause of disturbance, and this cannot be discounted within the instrument, it is sometimes an advantage to make the error so created self-recording. This is sometimes done in the case of micrometric photography, especially where for some reason film, rather than plate, has to be used. Two or more fiducial marks, often small "points" or lines, are marked on the film at the moment of exposure, either by means of supplementary focussed sources of light, or in the form of a "shadow-graph" of some small obstruction placed against the film. These marks are at a known distance apart when photographed, and any change in this distance, as measured on the film after development (due, e.g., to shrinkage of film), indicates the change of scale of the film between its exposure and the moment of measurement. This enables the true image size of other objects shown on the film to be correctly calculated. Again accurate mercury barometers are usually supplied with a thermometer, to enable the barometer reading to be corrected for the expansion of mercury and glass.

CHAPTER X

ACCURACY

Clearly, accuracy apart from precision can have no meaning; for precision is the quality of constancy as applied to the elements of an observation. The conditions favoring precision have been discussed at length; and, given a correct design, it has been seen that precision is not usually dependent on the quality of manufacture, except as regards sliding and rolling surfaces. In particular, precision can almost always be obtained independently of dimensional exactitude. But, although precision is a necessary, it is not a sufficient, condition for accuracy; for an ideally precise observation may still be burdened with systematic error.

This type of error was discussed in Chapter II, and not much more remains to be said; for the problems left are largely those of the shop rather than the draughting office. However even here the designer can help to a certain extent, and a few points will be considered.

NECESSARY AND MINIMUM CONDITIONS FOR ACCURACY

A very common source of expense and worry, in the manufacture of instruments, results from erroneously assuming that the accuracy of a measurement depends on some factor with which it has in fact no connection. Or alternatively choosing a method of measurement which does quite unnecessarily involve the accuracy of an additional element. This sounds elementary; but one of the most difficult stages in the design of an instrument is sometimes to determine the necessary and *minimum* conditions for accuracy. Optical instruments in particular supply several examples in which unnecessary accuracy has been involved.

EXAMPLE; CENTRING ERRORS

For instance it is generally known that in the case of instruments for measuring angles such as goniometers, surveyors' and astronomical instruments, etc., no error results if the graduated circle is moderately decentered, provided it be read at the two ends of a diameter at each setting and a mean taken (adding or subtracting 180° from one of the readings before meaning; this will be assumed in future). This saves any excessive centering accuracy; but at the same time a considerable fraction of the expense of such instruments is due to the supposed necessity for securing that the turning element shall be absolutely free from the least suspicion of shake in its bearings; this fault being held to be fatal to accuracy. It was Captain T. Y. Baker, R. N., of the Admiralty Research Laboratory, England, who first pointed out to me that no appreciable error need result even if this element be moderately incomplete.

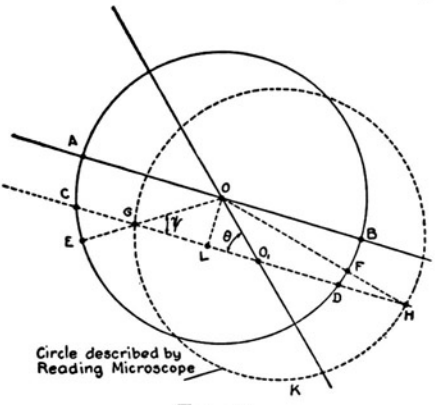

FIG. 54.

Thus, in Fig. 54, *ABD* represents a graduated circle with its centre at *O*. The moving member *AB*, also centred at *O*, is supposed to be sighted on some very distant object, such as a star or some collimated mark. Then imagine that the moving member is decentered, and now rotates about the fixed centre O_1; since it is still supposed to be sighted on the same distant object, *CD* will be parallel to *AB* and hence,

$$\text{Arc } AC = \text{Arc } BD.$$

Readings $$\frac{A+B}{2} = \frac{C+D}{2}.$$

Thus no error is introduced by any decentering when sighting on a distant object *provided the circle is read at its correct radius from O*. There are difficulties in the way of this, however, for the two reading microscopes travel round the circle *GHK* instead of *ABD;* and, if the reading is taken at the centres of their cross wires, it is the former circle on which they will read. The circle *ABD* might be indicated by engraving it on the graduations, cutting them at the point along their lengths where readings should be taken; but the engraved graduations are excessively fine, and an engraved line crossing them would spoil their quality at exactly the most important place. Consequently the position along each engraved line, at which the circle is read, is determined by the microscopes' graticules, and so follows the circle *GHK*.

The graduations read at *G* and *H* cut the true circle at *E* and *F* and the resulting error,

$$\epsilon = \frac{E+F}{2} - \frac{C+D}{2}.$$

Or, expressing this in terms of two arcs,

$$\epsilon = CE + FD.$$

In general, remembering that the decentering should be not greater than about 0.002 inch with a 10 inch circle, and, assuming very poor adjustment, the following approximate solution gives ϵ to a high order of accuracy; small arcs are taken as equivalent to their chords;

$$\begin{aligned}\epsilon &= CE+FD\\ &= 2\times CE\\ &= 2\times OL\times\frac{CG}{r},\text{ where } r \text{ is the radius of the circle,}\\ &= \frac{2\times OL\times LO_1}{r}\\ &= \frac{2\times OO_1^2(\operatorname{Sin}\theta\cdot\operatorname{Cos}\theta)}{r}.\end{aligned}$$

ϵ is evidently a quadrantal error which has maxima when $\theta=(2n-1)\ 45°$, and is zero when

$$\theta=N\times 90°,\quad N \text{ any positive integer.}$$

$$\epsilon\max=\frac{OC^2}{r}.$$

Very roughly, since $1''=\frac{1}{200{,}000}$, if errors due to centering are not to exceed 0.1 second of angle then,

$$\frac{OO_1{}^2}{r}\leqq\frac{1}{2{,}000{,}000},$$

or

$$OO_1\leqq\frac{r}{1{,}400}.$$

Thus with a 10 inch circle the maximum permissible decentering is about 3 thousandths of an inch. This represents very easy limits, especially for such a fine circle as is here presupposed; for the majority of instruments the decentering could well have two or three times this value.

Thus it is seen that, provided the decentering of a graduated circle does not exceed some definite fraction of its radius, depending on the accuracy required, no appreciable error in recording the angular direction of the moving member results provided that readings of a distant object are taken at the two ends of a diameter.

In practice the measurement of an angle involves the difference of two readings. Thus, Fig. 55, the angle α is measured as,

$$\frac{(A+B)-(A'+B')}{2}.$$

If the small circle LMN represents the limit of permissible decentering, given the accuracy required, any position such as CD parallel to AB, and cutting the circle LMN, will give the same reading as AB

$$\left(\text{i.e., within practical limits, } \frac{C+D}{2}=\frac{A+B}{2}\right).$$

Similarly any position $C'D'$ parallel to $A'B'$ and cutting LMN will give the same reading as $A'B'$,

$$\therefore \quad \frac{(C+D)-(C'+D')}{2}=\frac{(A+B)-(A'+B')}{2} \text{ very nearly.}$$

Or in words: within moderate limits, the accuracy of an instrument for measuring angles when sighted on distant objects is independent of the constancy of the axis of rotation.

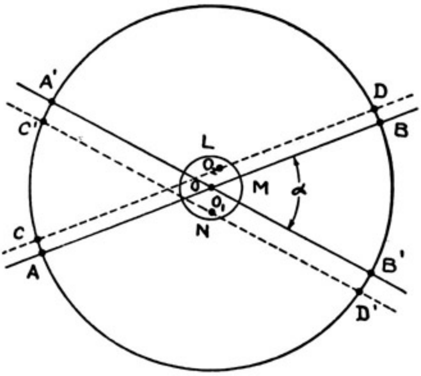

FIG. 55.

It is, of course, important that the two microscopes shall be truly on one diameter of the moving part, otherwise this error must be included in the total tolerance allowed for decentering; that is extremely easy to check and, with suitable design, to adjust.

This independence of a constant centre on the part of certain instruments should make for a cheaper and simpler design. Such instruments as astronomical telescopes, spectroscopes, and any instrument sighted on collimated or very distant objects come under this head; but the matter requires further investigation in the case of instruments sighting on near objects, such as surveyor's levels designed for work in mines. Probably the best cheap bearings for such instruments are ball bearings which, in their smaller sizes, are extremely inexpensively and accurately made. Some manufacturers sell these with three types of fit, easy, normal, and tight; and if the latter were chosen the possible uncertainty in the position of the axis would be something less than 0.0001 inch. This, of course, vastly exceeds the accuracy with which the instrument can be positioned in the first place, and the angle subtended by this length at 5 ft. is only about 0.3 second of angle. Hence no possible error of any sort can arise by sighting with an axis determined by ball bearings, provided that readings on the circle are taken at both ends of a diameter; the error due to neglecting this with a 6 inch circle is about 7 seconds of angle. This precaution is needed in any case to pick up centring error. Hence, with the possible exception of one or two of the very finest general purpose goniometers in the country, practically all angle measuring instruments would be equally good if fitted with standard ball bearings.

SECOND EXAMPLE

It seems astonishing that the above example is not better known, but the following is a more subtle instance in which an assumed dependence on an accurate element was discarded with great success.

J. Guild of the National Physical Laboratory devised the following apparatus to facilitate an investigation into the accuracy with which certain types of graticule lines, ordinarily used in microscopes and other optical instruments, could be set on object lines.

The following description is taken from Mr. Guild's account of the apparatus: [1]

The insensitivity of setting with an optical instrument depends not only on the insensitivity of the eye, but also on the magnifying power of the optical system. It was decided to make all determinations with the unaided eye. The angular constants so obtained have only to be divided by the magnifying power of any telescopic system to give the values which should apply when using such a system in addition to the eye.

The simplest procedure for determining the insensitivity of the unaided eye is to construct the graticule and the object line on a suitably large scale and arrange so that one is movable in front of the other at some distance from the observer. In practice, the drawback to such a simple arrangement lies in the fact that at any observation distance which is feasible in a laboratory experiment, very minute relative movements of the graticule and object are significant. For example, at a distance of 5 meters a relative movement of 0.024 mm. corresponds to an angle of 1 second. Very accurate construction of the carriage carrying the moving graticule, together with provision for accurately reading its position, would be necessary in order to preclude the possibility of instrumental errors of 1 second, an amount which is by no means negligible as the results show. Further, the provision of mechanism for operating the moving part, at a distance of 5 meters, with the necessary delicacy, presents considerable difficulty in view of the weight of the framework required to carry the graticule. These mechanical difficulties were finally overcome by the following device: Instead of moving the graticule in front of a fixed object line, both graticule and object line were attached to the carriage so that they moved together. They were separated *in the direction of the line of sight* by a distance equal to one-hundredth of their distance from the observer, who viewed them from a definitely located eyepoint. There was therefore an apparent relative movement of crosslines and object, due to parallax, equal to a hundredth of the actual movement of the carriage. This magnification of the actual movement required to produce a given angular alteration in the relative positions of graticule and object enabled the required delicacy to be obtained with relatively crude moving parts and second now required a movement of the carriage of 2.4 mm.

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[1] J. Guild, "The Insensitivity and Personal Equation Errors of Optical Settings", *Trans. of the Optical Society* (Great Britain), Vol. XXXI. 1929–30. No. 1.

A flanged brass ring of 70 cms. diameter and 5 cms. deep was supported on a carriage, fitted with rollers, which moved on a short length of photometer bench. The crosswires were attached to the front of the ring by thumb screws which could be inserted in any of series of perforations in the flange, thus enabling the wires to be fixed at any desired angle. The object wire was mounted vertically at the back edge of the ring, and was thus behind the plane of the graticule by the width of the ring, i.e., 5 cms. The object wire was kept taut by means of a short length of spiral spring to compensate for any slight deformation of the frame caused when mounting the graticule wires.

Field illumination was provided by a white paper diffusing screen illuminated from behind by a number of electric lamps so distributed as to give sensibly uniform brightness.

The interesting feature for us is the successive steps taken to ensure the necessary accuracy.

The type of graticule under investigation is used habitually in microscopes with a power of at least ×10 and often far more. Had a travelling microscope comparator been used, an angle of 1 second at the eye would have represented a movement of the microscope of about 0.000002 inch or roughly 0.1 μ with a power ×10, and proportionately less for higher powers; to measure such small movements accurately would have introduced great complications and expense, hence the power was reduced to unity; and, the microscope being dispensed with, the graticule and object line were placed at a considerable distance, in order still further to reduce the effective magnification. This well known device is habitually practiced in such work. Even this, however, still resulted in the uncomfortably small movement of only 24 μ per second of angle. Guild realized that he was not interested in the absolute positions of either the object line or the graticule, but only in their mutual relation; he then abandoned any attempt to measure the two former quantities, and devised his parallax method which gives the relation with extreme accuracy and with the roughest of measurements. Measurements of tenths of a second of angle were successfully performed with a hoop of brass whose only important dimension (its depth) could be in error by 0.1 mm. without affecting the results, a few bits of wire, a three wheeled trolley running on two rails actuated by a piece of string, and a metre scale. An inferior layout could have well required an extremely expensive apparatus, and the results would have been less reliable in the end. This brilliant result was achieved by realizing quite clearly what was required, viz., the *relation* between object line and graticule, and refusing to be led into measuring anything more.

Almost invariably a measurement is only required of some one quantity, and though it is sometimes necessary to measure a number of quantities accurately in order to determine the former, this state of affairs should always be viewed with suspicion. Similarly there is probably only one element on which the accuracy of measurement fundamentally depends, although other accurate elements may be incidentally required, but these should not present the bottle neck of the instrument, and should be kept as few as possible.

ACTUATING AND RECORDING MECHANISM

Most instruments consist of two main parts: an operating part, in which the instrument places itself into a given relation with the object of observation, and a recording mechanism which enables the observer to ascertain what the relation is; it is important that both these parts should be represented by short chains of elements, and that the latter part should not become the bottle neck if it can be possibly avoided.

There is often a great temptation, or even a necessity, to introduce a number of elements into the recording mechanism in order to bring the record into a convenient position for the observer. Sometimes the same necessity exists with regard to the controlling gear and it is a question as to how far the latter should be responsible for the record. As an example consider the actuating and recording mechanism for the travelling graticule in the apparatus just described. The graticule, a fairly bulky and moderately heavy component, was stationed 5 metres from the observer's eye and moved across his line of sight. Had the movements of the actuating gear been recorded at the observing end, this transmission would have had to correspond in its movements to the trolley carrying the graticule, and consequently be free from backlash, excessive whip, etc. Actually the transmission consisted of a piece of string, and the measurements were recorded by reading the position of a pointer attached to the trolley on a scale. This could easily be done by an assistant, or the observer himself, making use of a small telescope mounted so as to be permanently sighted on the scale. In this way the measurement was a direct record of the actual movement required, rather than of something else whose movements would have had to maintain some definite relation to the former; the actuating mechanism was relieved from any micrometric requirements.

The measurements required in this case were distinctly crude, but in the case of the Hilger photometric microscope already described the position of the microscope saddle on its slide had to be recorded with a least count of 1 μ; and, however desirable it might be to do this directly, questions of convenience indicated the provision of a more open scale. Consequently the required movement is deduced from the angular movement of the lead screw which is thus burdened with two distinct duties; first to move the microscope, and secondly to record this movement. The first duty demands no particular accuracy of form either in the screw or nut, and practically the whole expense of these parts is due to their function as recorders. Particularly in the case of heavy instruments it is often desirable to separate these two functions, in order to relieve the recording mechanism from the stresses unavoidably associated with the actual movements; the former can then be light and more or less kinematically designed, whilst the latter may be made with easy limits and designed for strength with ample bearing surfaces.

REQUIRED ACCURACY OF LINKS

Although the accuracy of an observation finally rests on the dimensional accuracy of one or more links, it does not follow that there is any predetermined ratio according to which an error in the one will produce an error in the other. On the contrary it is the business of the designer to so arrange the proportions of the elements that a given dimensional error in a link will produce the least possible error of observation. Moreover it is often possible to make the accuracy of observation dependent on the accuracy of any one or two of a number of links; and that one will be chosen which presents the fewest difficulties of manufacture or subsequent maintenance. Great scope exists for arranging the proportion of an element such that a low accuracy in some part will nevertheless lead to a high accuracy of the required relation.

THE RANGE FINDER

A modern range finder is an excellent example of this.[1] Without going into optical details, Fig. 56(*A*) shows the basic principle of the range finder. Light from a very distant object, such as a star, enters the pentagonals P_1 and P_2; *a* and *c* represent the principal rays. Each pencil before passing through an object lens is deflected through 90° by its pentagonal, and falls on a prism system which brings both images into the field of a single eyepiece. For simplicity the rays are drawn as falling on the opposite sides of the plane *E*. If the instrument is in adjustment both rays fall together as shown. Suppose, however, that some relatively close object is sighted; then, assuming that the direction of this object is such as not to disturb ray *a*, the other ray will lie along some direction *b*. Owing to the peculiar properties of double reflecting systems such as a pentagonal, discussed in the last chapter, the ray *b* will still be reflected through 90° and so will fail to coincide with the ray on *a* on *E*. The distance of the object viewed is clearly a function of *d*, the distance between *a′* and *b′*; and the task of the range finder is to measure *d*.

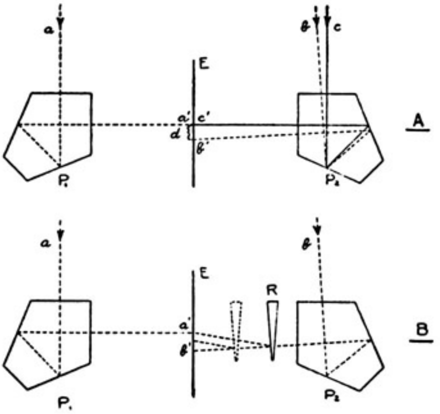

FIG. 56.

This might be done directly in some way, but *d*, for a 2 metre range finder at a range 2,000 metres, would only be 0.1 mm.; and the difference between d when ranging on two objects, the first at 2,000 and second at 1,900 metres would only be $5\ \mu$. Hence it would be difficult if not impossible to construct an accurate and handy range finder on these lines. Again one of the pentagonals, say P_2, might be replaced by a plane mirror, in which case the ray b would not in general be reflected through exactly 90°, and d could always be made zero by suitably angling the mirror. The precise angle through which the mirror is turned would then be a measure of the range of the object set on. It can easily be shown that in order to rival the accuracy of a modern range finder the mirror's position would have to be determined each time to an accuracy of about $\frac{1}{20}$ second of angle—an impossible proposition.

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[1] See pamphlet entitled "The Modern Range Finder", by Frederic J. Cheshire, for an excellent elementary description of this instrument. Printed under authority of H. M. Stationery Office by Messrs. Harrison and Sons, London, 1917.

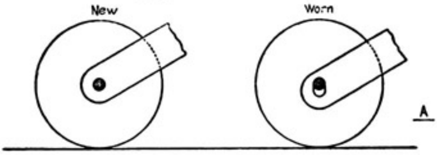
Various means are adopted to avoid these difficulties and the following, shown in Fig. 56(*B*), is one very successful arrangement. A weak prism is interposed in the ray b, between the pentagonal and the plane E, and has the effect of bending the ray b in a direction tending to compensate for its displacement at E. The closer the prism is placed to E the less this compensation, and vice versa. The instrument is then adjusted for infinity with the prism R at the extreme left hand end of its run, and, when taking a range, is moved to the right until b' coincides with a'; the position of R is then a function of the distance of the object ranged. The prism R carries a scale which reads directly in length (yards in the case of an English instrument.)

In practice the length of travel available for the prism depends on the particular model of the instrument and will be several inches at least. This is determined solely by the strength of the prism and the closest range to be catered for. Consequently it is an easy matter to arrange that a shift of the direction of b, corresponding to a fraction of a second of angle, shall require a relatively coarse movement of the prism by simply making the latter weak enough. What is required is a high degree of constancy in the power of the prism, and in the nature of things this is easily achieved.

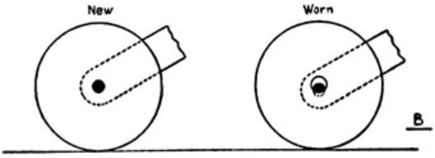
ELEMENTS WEARING TRUE

Elements vary greatly in their wearing qualities, not only as regards the amount of wear suffered but also as regards the influence of this wear on accuracy. It has already been seen that kinematic design does much to achieve accuracy in spite of wear by maintaining precision. This is not the only factor, and much can be done by a correct proportioning and arrangement of the parts.

Sir Horace Darwin in an article entitled "Scientific Instruments, Their Design and Use in Aëronautics"[1] gives a very simple example.



Spindle fast to Wheel



Spindle fast to Body

FIG. 57.

Take the case of wear in a wheelbarrow. The axle of the wheel usually consists of two round iron pins running in holes in wooden rails forming the frame of the wheelbarrow (Fig. 57*A*). Both the wood and the pins wear; the pin gets smaller but keeps circular, and wears its way into the wood and always fits it properly on the side where pressure is taken. The wheel will work perfectly till either the holes break out of the wood or the pin wears down very small and itself gives way. But sometimes the axle is made differently, an iron rod is fixed to the two wooden rails and passes through a hole bored along the centre of the wheel (Fig. 57*B*). With use the iron rod wears on the under side and does not remain circular, the hole in the wheel gets larger; the result is increased friction and a loose and shaky bearing.

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[1] *The Aëronautical Journal*, July, 1913.

Other examples are: the balancing of rotating parts to relieve pressure and hammering on the bearings; the use of slightly conical bearings in place of plane journals to enable wear to be taken up; the centre bearings of theodolites, goniometres are designed in this manner. Again, when one part slides on another some wear is inevitable; but the truth of the two surfaces is often approximately maintained if their proportions are such as to ensure that no part of either is entirely unused. This means that at different moments each should overhang the other.

CHAPTER XI

THE PLANNING OF AN INSTRUMENT

As an outcome of a preliminary survey, a particular type of instrument will be adopted for detailed design and manufacture; although it often happens that previous experience, or the circumstances of the case, are sufficient to indicate the sort of instrument required from the outset. This chapter will be devoted to discussing the planning of a particular type of instrument.

EXAMPLES: MEASURING MICROSCOPES

By way of illustration the design of measuring microscopes will be considered and various alternatives reviewed.

Measuring instruments of this general type vary from microscopes mounted on a simple slide reading to tenths of a millimetre by vernier, to highly complicated temperature controlled instruments weighing several tons and designed for some one purpose such as the checking of ultimate standards of length. We shall mainly consider the design of instruments lying midway between these two extremes, and intended for general use with a least count of about 1 μ and a travel of a few centimetres.

The fundamental requirements of a travelling microscope are precisely that the latter should travel, with respect to the measured object, in an assigned direction, in amounts under strict control, and capable of accurate evaluation. The first point to notice is that the movement required of the microscope is relative to the object and to nothing else; hence it cannot matter whether the microscope moves relatively to the stand, the object remaining stationary, or vice versa; or both object and microscope could be arranged to move. This does not necessarily imply that these alternatives will result in equally convenient or accurate instruments; but they all satisfy the geometrical conditions of the problem, and so fall to be considered. This raises a point of very general application; an element defines a relation between two links, and where this relation is one of motion it will often be found that by a slight readjustment of the preceding and succeeding elements this relative motion can be secured by moving either or both links relatively to the "stationary" framework. Thus if a nut is required to travel along a screw it is, geometrically, a matter of indifference whether the nut is rotated on a screw held stationary, or vice versa; or whether both turn; similarly any one of these three cases is compatible with either or both links having a motion of translation.

Sir Horace Darwin gives excellent examples of the possibility of "reversing the parts of a machine", as he terms it, in an article printed in the *Aëronautical Journal* of July, 1913:

> An improvement in the design of a machine can often be made by reversing the relative position of two parts of it, or the part that moved can be fixed and the part that was fixed can be made to move. This reversal makes it possible to compare two or more methods, and it is then easy to see which is best. It is advantageous that the "survival of the fittest" should take place early in the life of the machine, and by this means, in fact, it takes place before the design is completed.
>
> In a cutting tool it is a question whether the tool or the work should move; sometimes one is best and sometimes the other. In the wheelbarrow [1] it is easy to see which is the best design, and if the designer had deliberately considered whether the iron pins should turn in the wooden rails or whether the iron bar should be fixed, the [latter]

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[1] This example is given in full in Chapter X.

bad design would never have been made. It is surprising how often this reversal is possible and advantageous, and how difficult it is to realize that it is possible. We are so familiar with a clock in which the frame remains at rest and the hands move that it requires a considerable mental wrench to realize that it is possible and in some cases better that the clock itself should revolve and the hour hand remain at rest. But in recording apparatus it is usual to fix the clockwork in the rotating drum carrying the paper, and to prevent rotation of the hour hand spindle. If the clock is at rest, as is usual with clocks, the axis must rotate, but if the axis is fixed the clock itself will be driven round. Another reversal of the usual arrangement is the Gnome engine with its fixed crank and rotating cylinders.[1]

Returning to the measuring microscope, it is very usual to produce the relative movement between the microscope and object by the rotation of a screw relatively to its nut. For one thing this arrangement gives a very great reduction in motion between the hand and the moving member, and thus secures great smoothness in action, and the possibility of fine control. On this understanding, and neglecting all considerations other than the relative motion of microscope and object, the following alternatives present themselves:

i. The microscope slides on the frame, the object remaining stationary
ii. The object slides on the frame, the microscope remaining stationary
iii. Both object and microscope slide on the frame.

Consider the first alternative, namely a fixed object and a moving microscope; either the nut or the screw may move along the frame with the microscope; and in either case it is possible to arrange for the nut or the screw to be the rotating member.

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[1] When this was written the Gnome engine was very frequently used in aëroplanes.

Exactly the same remarks apply to the second alternative by substituting the object for the microscope. The third alternative will be considered later, but, neglecting this, the possibilities are:

i. The microscope slides on the frame, the object stationary:

A. Nut translates with microscope $\left\{\begin{array}{l}\alpha.\ \text{nut rotates}\\ \beta.\ \text{screw rotates}\end{array}\right.$

B. Screw translates with microscope $\left\{\begin{array}{l}\alpha.\ \text{nut rotates}\\ \beta.\ \text{screw rotates}\end{array}\right.$

ii. The object slides on the frame, the microscope stationary:

A. Nut translates with object $\left\{\begin{array}{l}\alpha.\ \text{nut rotates}\\ \beta.\ \text{screw rotates}\end{array}\right.$

B. Screw translates with object $\left\{\begin{array}{l}\alpha.\ \text{nut rotates}\\ \beta.\ \text{screw rotates}\end{array}\right.$

At this stage it would be well to sketch these eight alternatives, without any regard to proportion, in order to visualize the type of arrangements involved in each case. The first four alternatives are diagrammatically sketched in Figs. 58 and 59, and the second four in Figs. 60 and 61. No attempt has been made to design the form or the correct constraints of any of the parts, the object being merely to show the alternative motions in the simplest possible way.

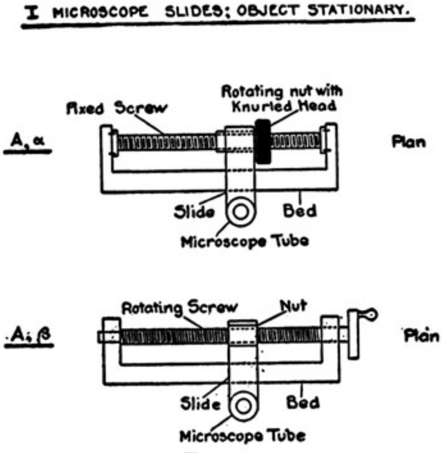


FIG. 58.

Several things can be seen by glancing at these diagrammatic sketches. First consider ease of manipulation. Two of the cases, *iA*α and *iiA*α, show a nut travelling with the moving member and rotating; as drawn, the hand control, a knurled head, will alter its position along the length of the instrument according to the setting of the moving member. This is a disadvantage since it is unlikely that all positions will be equally convenient to the observer, and even if they were, any change, unless the travel be very short, will divert his attention from more important matters by causing him to seek the control every time he brings up his hand. This design also suffers from another defect, namely, that the control is almost necessarily inconvenient for the reason that the position of the nut on the screw prevents the observer from holding the control with his hand covering its end face; Fig. 62 illustrates this grip which is generally found to be by far the most convenient and permits of a delicate adjustment. It is true that these difficulties could be overcome at the cost of additional complication, but such a control would have to be very well made if it were not to be either rendered

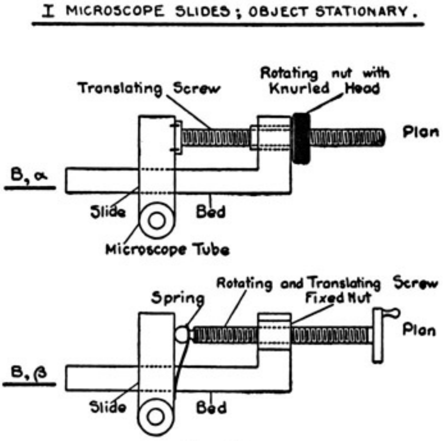

I MICROSCOPE SLIDES; OBJECT STATIONARY.
Translating Screw
Rotating nut with Knurled Head
Plan
B, α
Slide
Bed
Microscope Tube
Rotating and Translating Screw
Spring
Fixed Nut
Plan
B, β
Slide
Bed

FIG. 59.

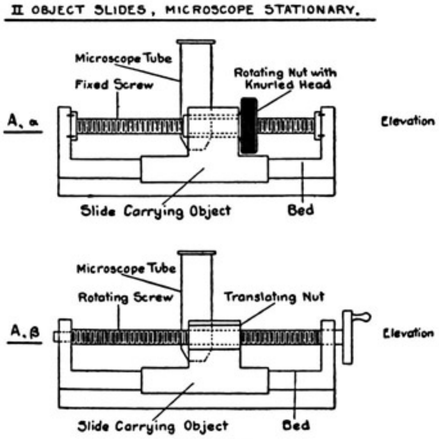


FIG. 60.

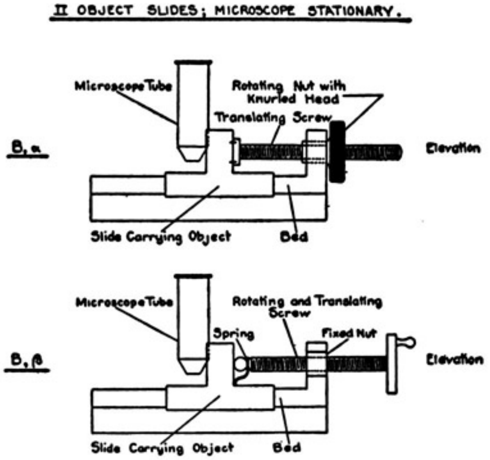


FIG. 61.

heavy by friction or suffer from backlash. Consequently, unless some very strong reason for adopting the rotating and translating nut subsequently appears, $iA\alpha$ and $iiA\alpha$ can be eliminated from the practical alternatives.

Next consider $iB\alpha$ and $iiB\alpha$; except that the control does not change its position, the same general remarks hold; though, if only a short travel, say 4 or 5 cm., were required, the depth of the nut (not necessarily threaded throughout its length) might be sufficiently great to prevent the screw from coming out beyond its end face. The nut in any case would be expensive since the concentricity of the inner thread and its outer bearing surface is vital. This, for the present at least, eliminates those alternatives in which the nut rather than the screw rotates. Of the four cases in which the screw rotates, $iB\beta$ and $iiB\beta$ suffer from the movement of the control, and this becomes especially undesirable in the case of a long travel.

Thus, from the point of view of ease of control coupled with simplicity, the rotating screw systems are superior to those having a rotating nut, and of the former $iA\beta$ and $iiA\beta$

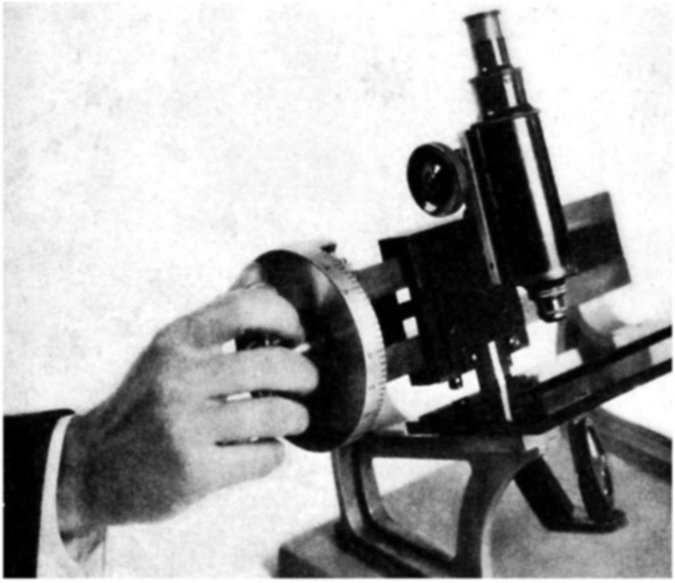


Fig. 62.

seem the best. Since it is vital that the screw should not be distorted nor the slide forced out of truth, the next thing to consider is which systems most readily permit of the necessary relations without overconstraint.

$iA\alpha$ and $iiA\alpha$ are identical from this point of view, and it would seem difficult not to overconstrain the screw and the slide, unless the housing of the former were decidedly complicated. Probably the simplest arrangement would be to fix the nut to the slide with freedom to rotate, have one end of the screw free, and couple the other end to the frame with an Oldham's coupling, or its kinematic equivalent, together with a keep plate to prevent end movement: Fig. 63 shows this diagrammatically. This is not absolutely free from overconstraint, but with good design could be made very nearly so; it would be expensive and opens the way to backlash. The two other arrangements in which the nut rotates, namely, $iB\alpha$ and $iiB\alpha$, present exactly the same features from the point of view of constraint, except that the frame and moving members have changed place, and the same solution would be applicable.

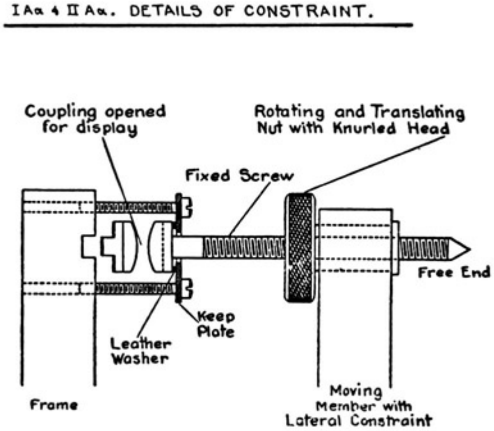

IAα & IIAα. DETAILS OF CONSTRAINT.
Coupling opened for display
Rotating and Translating Nut with Knurled Head
Fixed Screw
Free End
Keep Plate
Leather Washer
Frame
Moving Member with Lateral Constraint

FIG. 63.

Turning to the four arrangements in which the screw rotates, $iA\beta$ and $iiA\beta$ are precisely similar, and in practice it is quite usual to have the far end of the screw free, as in Fig. 63, and constrain the screw completely both at the nut and in its remaining bearing, probably making the latter short. Alternatively, the nut can be given side float without introducing unwanted rotation. Again the rotating and translating screw arrangement, as shown in Figs. 59 and 61, can very easily be designed as complete, but not overconstrained, elements. Of course most of the overconstrained designs could be saved by housing the two fixings of the screw (one on the frame and the other on the sliding member) in gimbal rings or their kinematic equivalent, but this solution is heroic, resulting in expense and the possibility of lost motion.

So far no consideration has been given to the third major alternative, namely that in which the object and microscope are both capable of sliding on the frame. This can hardly fail to be both complicated and expensive, and no difficulty which could possibly be met by this expedient has yet arisen. In the early stages of design no alternative should be definitely and finally cast away, but the more obviously complicated possibilities can be held in the background until some difficulty presents itself which upsets the original scale of values, and necessitates a new survey of the situation. As we shall see later there are occasions when it becomes desirable to slide both members.

For the present we will confine ourselves to the two major alternatives of a sliding microscope or a sliding object, and their variants. Mention has been made of the possibility of backlash with the implication that this would be a defect, but it has already been explained earlier in the book that this would not directly lead to error, for in all measurements with measuring microscopes the backlash is always taken up in the same direction; however, a more subtle error results from the lack of proportionality between the movement of the controlling hand and that of the sliding member; for this causes a loss of delicacy of control and an irritating diversion of attention.

One way of reducing the chance of backlash is to spring load the sliding member in such a manner as to put the screw into tension or compression; usually the latter, since this assists in holding the parts together and so simplifies the design. Not all forms of backlash are eliminated by this device, as for instance in the case of the coupling shown in Fig. 63, but all backlash resulting from unwanted freedom along the direction of travel would be wiped out. By preventing the screw from ever being in tension, a spring loaded sliding member permits of a beautiful arrangement in the cases where the rotating screw also translates, *iBβ* and *iiBβ*; this is shown in Fig. 64. I do not know who originated this design, but the earliest instruments so fitted that I have seen were made by the Cambridge Instrument Company (England).

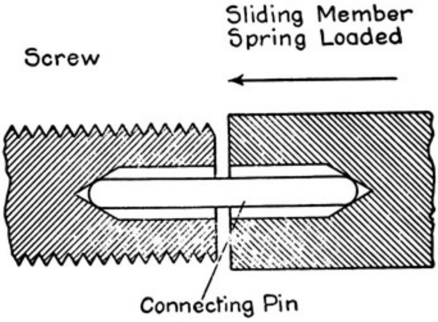
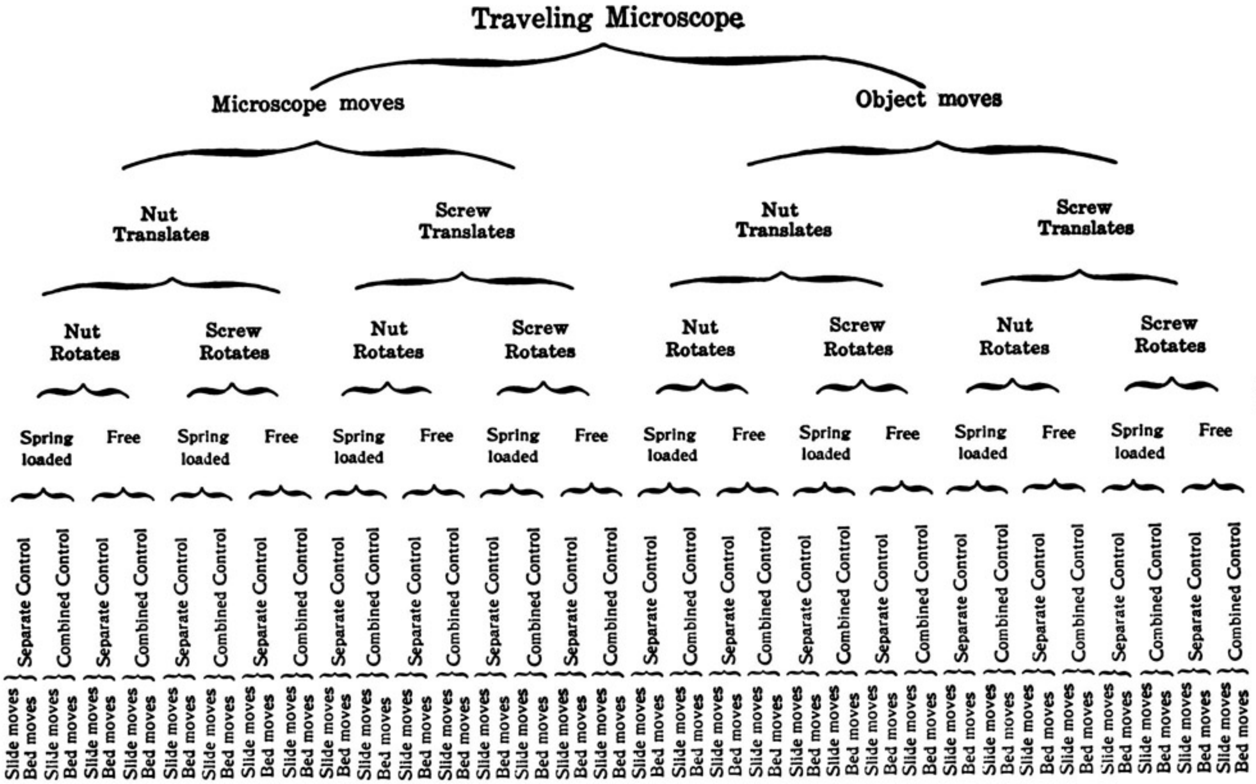


FIG. 64.

Unfortunately the use of a spring to eliminate backlash has one limitation in that in its simplest, and so best, form it cannot be conveniently used where the maximum travel of the sliding member is more than a small fraction of the instrument's overall length. Clearly the force of the spring in its least strained position must be sufficient to overcome the likely forces of friction with an ample margin for safety; equally it is undesirable that this least force should be greatly exceeded when the spring is in its position of maximum strain; consequently the spring, which, in the absence of gearing or linkwork, will take the form of a helix, must be long compared to the length of the maximum travel. The objections to any great variation of spring tension are twofold; first that the greater force in some positions of the slide will lead to an unnecessarily stiff control, and a variation in this stiffness leading to different sensations when setting on the two ends of the object; secondly, the variation in force may lead to minute, but significant, dimensional changes in the parts affected.

This limitation could of course be overcome by linkwork or gearing; one such example will be described later, as well as other instruments in which the loading is obtained by gravity acting on the moving member, or produced by a weight hanging over a pulley wheel. Yet another alternative presents itself; for the recording device may, but need not be, performed by the principal lead screw actuating the sliding member. There is something to be said for and against either system. In practice where a least count of 1 β is required the simplest form of measuring device is usually a screw, and it seems redundant to arrange for two screws if one can perform both functions, as the former plan involves a double manipulation. As against this a lead screw intended merely to move the slide would only have to be made to moderate limits of accuracy, and so would not be expensive; it performs all the heavy work and so suffers the bulk of the wear, whilst the micrometric screw could be lighter and would be less liable to deterioration with time. In Chapter II (Fig. 6) I have already described such an instrument designed with a separate measuring screw.

Again the "slide" and the "bed" can be interchanged in any of the above designs. Referring for a moment to ordinary machine tool practice, it is usual for the slide to have bearing surfaces which are short compared to the bed on which they slide; consequently the areas of contact between the two are fixed with respect to the slide but vary on the bed according to the setting; often, however, this arrangement is reversed, or a compromise is effected by making slide and bed of about the same length, in which case each overlaps the other on occasions. I shall use the term "slide" to represent that component relatively to which the areas


Traveling Microscope
Microscope moves
Object moves
Nut Translates
Screw Translates
Nut Translates
Screw Translates
Nut Rotates
Screw Rotates
Nut Rotates
Screw Rotates
Nut Rotates
Screw Rotates
Nut Rotates
Screw Rotates
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of contact are fixed, the "bed" being the other component, irrespective of which may be the moving member.

Thus, as Table VII shows, up to the present 64 thoroughly practical alternative arrangements for a travelling microscope have been envolved and the possible variations of detail within these limits are literally innumerable; whilst, had the possibilities of a moving microscope and object been considered, either member might have been designed independently to move in any of 32 ways, and so 32^2 or over 1,000 practical alternatives would have presented themselves at this stage, including possibly some more which would result from moving both members by some form of combined control.

It would obviously be impracticable to consider the comparative merits of every possibility by examining each individually, and fortunately this is not really necessary. In the present example, for instance, the cases arise from all the possible ways of combining the six following alternatives.

1. Microscope or object moves.
2. Nut or screw translates.
3. Nut or screw rotates.
4. Moving member loaded or free.
5. Moving and measuring controls separate or combined.
6. Slide or bed moves.

Since these alternatives are partly independent they can be considered separately; although in the details of design they would mutually interact, and this necessitates a series of explorations amongst their permutations. In particular, speaking from experience, it is fatally easy to incline towards one case after an adequate survey, which has excluded a particular alternative for good reasons; then to find that attention to the details makes some other class of cases more suitable, and to determine the design with the fixed idea that the excluded alternative has been adequately disposed of; whereas the change of opinion resulting from further investigation may have made this the most desirable after all. In fact no alternative should be finally barred until the particular case, complete in all its major details, has been irrevocably adopted. This is very obvious and easy to write but the most difficult advice to follow in practice; for the cases are too numerous to review systematically, and the designer has to rely on a general flexibility of mind coupled with common sense and experience.

Only some of the larger alternatives and problems have been touched on, but it may help if a number of travelling microscopes of good quality are examined and their various features contrasted.

1. Adam Hilger: Measuring Micrometer

Figure 65 shows a general view of one model of Messrs. Adam Hilger's "Measuring Micrometer". This instrument measures lengths up to 150 mm., 6 inches, with a least count of 1 μ; and the maker guarantees the accuracy of the absolute pitch of the lead screw to one part in 20,000, whilst measurements made on the instrument are stated to be consistent with each other to 0.5 μ. Certainly the instrument is made with the very finest workmanship, and one I have known for over eight years has been in frequent use throughout the whole of that time without showing any signs of wear or deterioration.

The base plate is a solid iron casting, to which are screwed the two brackets carrying the stage and the bed *B*, on which the microscope slide *S* moves. It will be seen that this instrument is of the type in which the screw rotates and the nut, fast to the microscope slide, translates. The bed, machined out of one solid piece, is of *V* form; whilst the slide is made in two pieces to permit of assembly, and presumably to assist machining operations. This element is strongly reminiscent of ordinary machine tool practice.

The steel screw has an external diameter of 10 mm. and is threaded with a pitch of 1 mm. One end, the right in the photograph, is free whilst the other is constrained by a journal and a thrust bearing. The brass nut has a length of 33 mm. and is made in two halves, being divided along its length. It will be noticed that the screw and hence the slide are overconstrained.

The measuring mechanism is combined with the actuating gear; it consists of a drum *D* fast to the screw which can be read by the help of the vernier *V*. This places the responsibility for accuracy on the screw and nut, and these are made with great skill to remarkably fine limits.

The slide *S* carries the microscope tube *T*, in which slides the microscope *M*; the former is split and grips the latter without shake. Figure 66 shows the details, the microscope is focussed by either of the two knurled heads *K*, fast to a spindle carrying a pinion, which engages the rack *R* on the microscope. It will be noticed that the teeth of the rack are cut diagonally to prevent any suspicion of jerkiness.

The microscope is normal to the plane of the stage which latter has an aperture cut out along its length, directly under the microscope, so that the object can be illuminated by reflected light from above or transmitted light from below. To assist the latter, a circular mirror is carried underneath the stage on a universal mount which is attached to the slide *S*, and so moves with the microscope. For some reason this was removed when the photograph was taken, but is shown in Fig. 62. As can be seen, the whole instrument is so designed as to be tilted slightly towards the observer, who would be behind the instrument in Fig. 65. This tilt considerably increases the comfort of the observer by avoiding the necessity of looking directly downwards.

Some of the possible systematic errors to which this in-

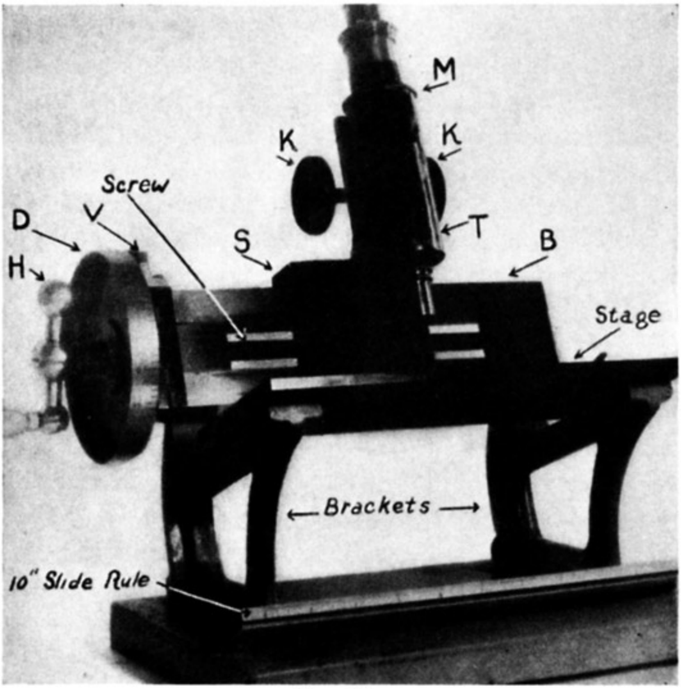

FIG. 65.

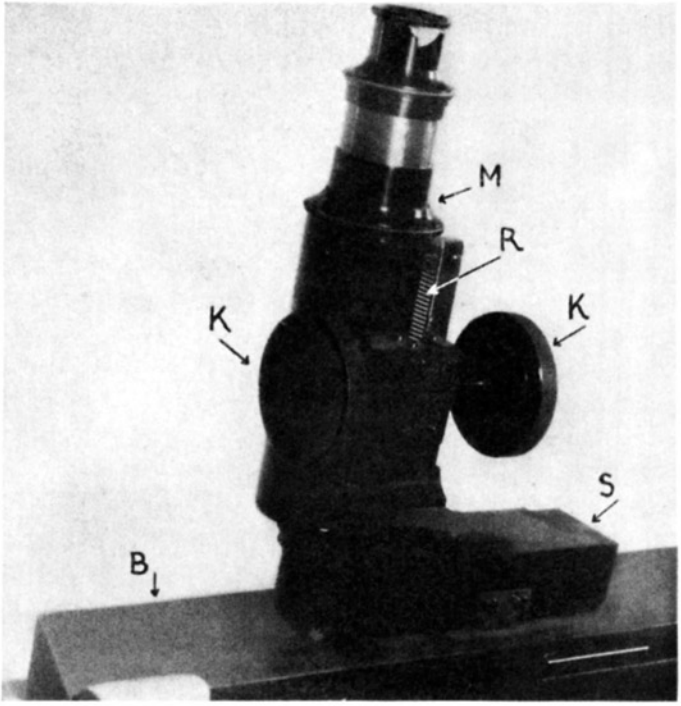

FIG. 66.

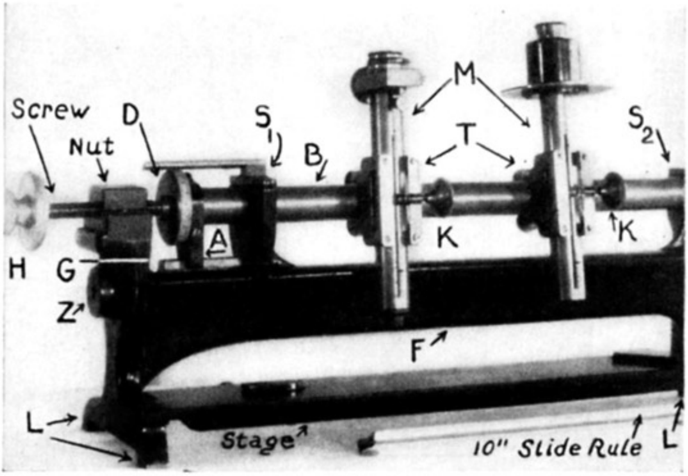

FIG. 67.

strument is liable were discussed in detail in Chapter II, but the possibility of erratic errors, in the element comprising the slide and bed, is by far the most serious danger. The design is such that the completion of this element entirely rests on the quality of the workmanship; four surfaces in three different planes are involved in each link, and the slightest error of workmanship might cause wobble between the two; moreover the proportions of the slide and its fittings are such as to enhance the errors to be feared. The screw and nut are capable of measurements consistent to 0.5 μ, or about $\frac{1}{50,000}$ of an inch, but it will be noticed that the length of the slide is comparable to the distance from its centre to the stage, and so a wobble of $\frac{1}{50,000}$ of an inch about an axis normal to the paper would produce a like error of measurement. Similarly the distance between the optical axis of the microscope and the plane of the bed is more than half that of the slide's length, and so an error of over 0.5 μ would result from a wobble of $\frac{1}{25,000}$ of an inch about an axis parallel to the optical axis. It has already been noticed that the screw and slide are overconstrained; but in one respect at least they are happily placed, for, by placing the screw along the centre line of the bed, its thrust on the slide is symmetrical about the surfaces of sliding contact, and no couple tending to twist the slide with respect to its bed is generated. Another vital requirement is that the stage should be rigidly held with respect to the bed, and the arrangements for securing this also depend entirely on workmanship. For the brackets each have one long but narrow foot which is held to the base by screws, whilst the bed and stage are screwed to similar narrow arms, as the photograph shows. It would seem as though with continual use some of these screws might permit a small shake to develop which would render the instrument unreliable.

Having known one of these instruments for many years, and used it very frequently, I must admit that not one of these defects has ever appeared; moreover, this instrument has such an excellent reputation that I believe my experience to be typical. The explanation is of course to be found in the quality of the workmanship, for which this firm has a world wide reputation amongst instrument makers and scientists generally. Workmanship has partly taken the place of design, and, whilst the product is eminently successful in use, it is fair to suggest that the same result might have been obtained with a smaller expenditure of human skill had the design been differently conceived; moreover, in the ordinary course of things very few firms can hope to have such skill at their command.

Lately Messrs. Adam Hilger have produced another design which I shall describe later, and we may conclude this description of the older instrument by noting some respects in which the design is decidedly happy. First the instrument is extremely simple, contains the very minimum of parts, and these few are of robust section; there is an entire absence of little devices, which look so attractive at first sight and often give trouble in the end. Then the length of travel of the slide, 6 inches, is very good for such a compact instrument; the "ten inch" slide rule in the photograph (Fig. 65) is about eleven inches in length. Finally, the control *H* deserves especial mention; it is held not by the handle which can be seen at its lower end, but as in Fig. 62, and is the very best control I have yet seen. It permits of a comfortable hand position and delicate actuation; whilst the handle does not get in the way and is useful for traversing the microscope rapidly from one end of the object to the other. Incidentally it is interesting to notice that a movement of one millimetre per revolution proves very suitable for settings with a least count of about 1 μ. If the actuation is too coarse attention is diverted from the sight to the hand, which has to make uncomfortably fine movements; and precisely the same result occurs if the actuation is too fine, since the hand has to make unpleasantly large movements to produce any visible result. In practice there is an optimum speed for any given hand action, and also an optimum speed of movement in the apparent field of the microscope, for a given type of setting; and it is important that the first should produce the second.

2. Cambridge Instrument Company: Measuring Microscope

A very interesting instrument is the "Cambridge Measuring Microscope" made by the Cambridge Instrument Company of Cambridge, England. This instrument, a general view of which is shown in Fig. 67, is a complete contrast to the last in every possible way, and is designed on kinematic principles throughout.

In this instrument the microscope moves whilst the screw rotates and translates, the nut being fast to the frame. The moving member is a "bed" in the sense defined above, and is spring loaded; and, as in the last instrument, the measuring device and actuating control are combined.

The lettering in the photograph is in line with that of the previous instrument to assist comparison. The frame *F* is a substantial T-sectioned casting in one piece with the three legs *L* and with the stationary "slides" *SS*. These latter can be best seen in Fig. 68, where the microscopes are removed, and consist of two V-guides, each containing two locators for the tube *B*, and one restoring force. These are illustrated diagrammatically in Fig. 39(*A*), Chapter VII, where they are discussed. The moving "bed" *B* is spring loaded onto the screw, and connected to it by the device shown in Fig. 64; Fig. 69 shows this, *P* being the connecting pin held in compression between *B* and the screw by a tension spring. This latter is housed within the hollow tube *B* and attached to the free end of the latter and to a pin projecting from *S*, Fig. 67, through a slot in *B*. The moving "bed" is prevented from rotating by an arm *A*, Figs. 67 and 69, fast to the former and bearing against a guide *G* cast with the frame. The arm is held up to the guide by a leaf spring fast to *B*, and bearing against the opposite side of the guide. The screw, which is threaded with a pitch of 1 mm., rotates and translates in the fixed nut and is not otherwise constrained; at one end the screw carries the knurled heads *H*, the larger of which is used for making a setting and the smaller for fast traversing; at the other end of the screw is mounted the reading drum *D*, graduated to 10 μ, and capable of being read to 1 μ by estimation. The total translation of the screw is only 40 mm., or 1.6 inches.

As is shown in Fig. 67 two microscopes are supplied with the instrument and are attached to *B* by means of the clamps *T* (see also Fig. 70 which shows one microscope in detail, and Fig. 71 where the parts are photographed separately). *T* is a casting, formed to perform two distinct functions; first to be adjustably attached to *B*, and secondly to hold the microscope without shake and yet with the freedom required for focussing. In both cases the method adopted is kinematically identical, and is equivalent to the means by which the moving "bed" *B* is held in its "slides" *S*. Thus in each case *T* presents to the tube—*B* or microscope—two V-slides each with two locators and one restoring force common to both V's. In the case of the attachment to *B* the restoring force is produced by a bar pressed onto *B* by two thumb nuts (clearly seen in Fig. 71); whilst the microscope is forced into its V's by a small groove turned out of the spindle fast to *K* which wedges over a semicircular rod attached to the microscope. The spindle is forced onto the microscope by locating a small sphere at its free end in a suitable hole, whilst a leaf spring forces back the spindle on the side carrying *K*. This serves to prevent any rotation of the microscope about its axis, and, by turning *K*, raises or

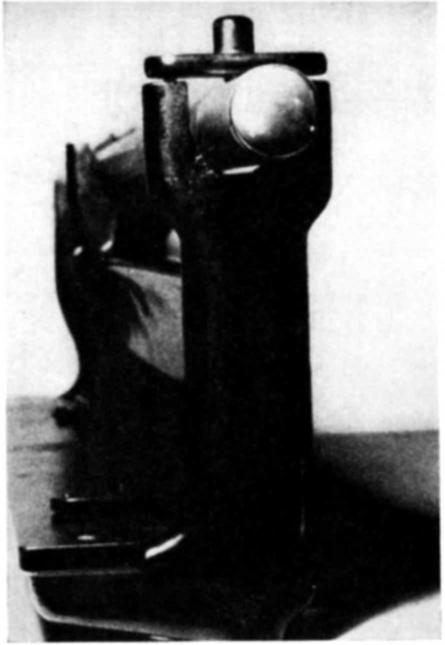

Fig. 68.

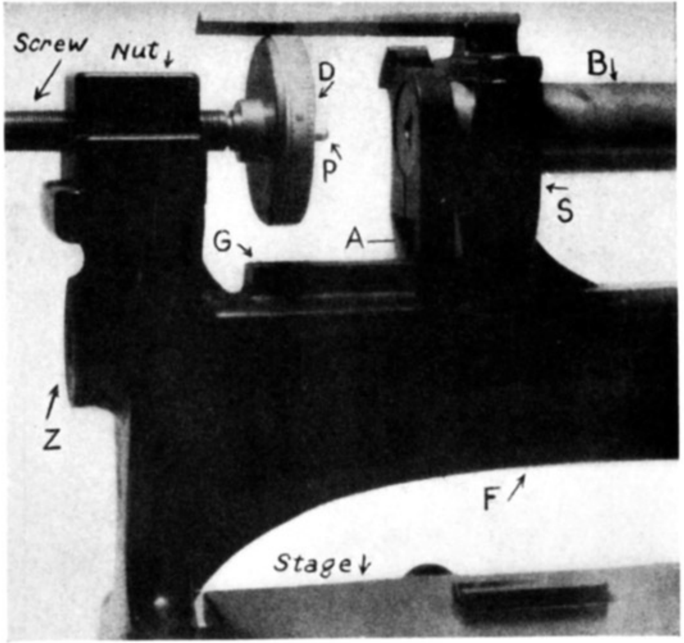

Fig. 69.

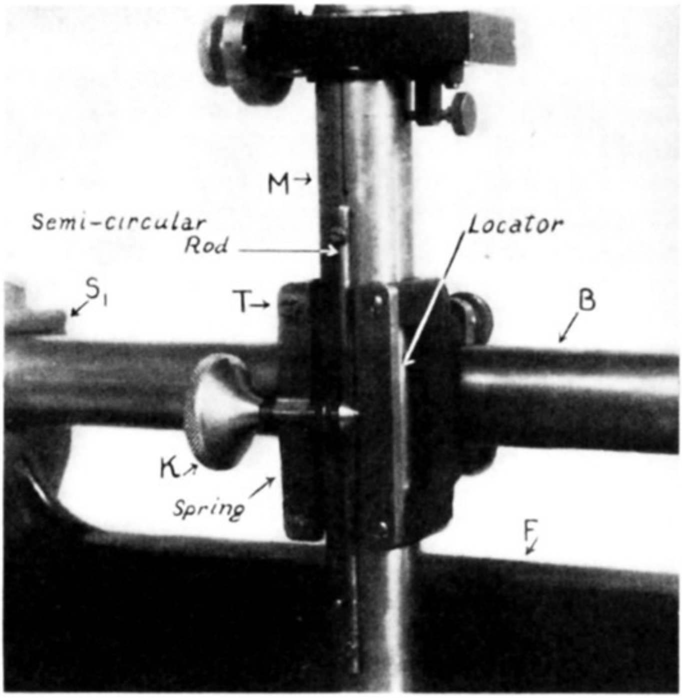

FIG. 70.

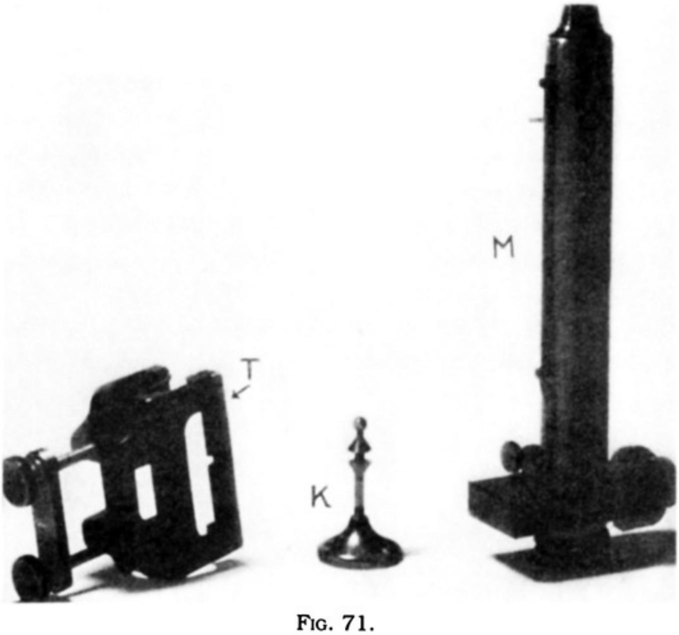

FIG. 71.

lowers it. Considerable trouble has been taken by microscope makers in the past to devise a focussing mechanism which shall be light in action, free from shake, and show no signs of jerkiness; the device just described is by far the best and cheapest solution I have seen; though it is doubtful whether it could be made to give a sufficiently fine motion in the case of really high-powered microscopes, at least in the form shown here.

The microscopes can evidently be independently fixed in any position along *B*, and, either facing the stage, or pointing in some other direction to measure an object independently mounted. Either microscope can be used alone, the other being ignored; or they may be used in combination. Thus they can be set at some definite distance apart, as determined by "cutting" on an object of known length, and so used together to measure a second object with a length of over 40 mm.; or, if a comparison between two similar objects be required, each microscope can be simultaneously cut on one end of each object and the difference of readings of the two cuts at the other end recorded. Again the two microscopes could compare two independently mounted objects, if their relevant lengths lay parallel, by pointing in different directions. Measurements of lengths greater than 4 cm. are facilitated by using a glass graduated scale supplied by the makers. Such settings are greatly assisted by the micrometer head of the left microscope (Fig. 67) which enables the cross wires to be brought directly onto the object without touching *H*. This micrometer head, which can be turned into any direction around the optical axis, can also measure short lengths at any angle relatively to the object's principal length. The other microscope carries a rotating head capable of reading angles to 10 minutes by vernier.

Finally a small three legged stand is supplied which can be screwed into a boss (*Z* in Figs. 67 and 69) at the end of the frame, the whole instrument is then stood upright and measures lengths vertically in the manner of a cathetometer.

As compared with the Hilger "Measuring Micrometer" the Cambridge instrument possesses a remarkably wide range of possible uses and comes very near to being an ideal instrument for general use. It has, however, two defects: first its maximum convenient length of measurement is only 40 mm. as against the "Measuring Micrometer" which has a range of 150 mm.; this is proportionately less meritorious when the former's greater overall length is considered; the same slide rule in Figs. 65 and 67 assists comparison. This is only partially discounted by the rather cumbersome process, described above, by which the two microscopes taken together can lengthen the range. Secondly the stage is solid and transmitted light can only be used by mounting the object independently. Incidentally the stage has a hole, slot, and plane to carry an adjustable platform, not shown. The details of the design are particularly interesting and follow this firm's usual practice of designing strictly according to kinematic principles; each element being fully, but never over, constrained; hence the completion of each element is independent of the workmanship and the most serious source of erratic errors eliminated. Moreover, the actual proportions of the links are such as to minimize systematic or other errors, such as might arise from imperfections of workmanship. The most dangerous element in such an instrument is, as already stated, that containing the slide and its bed; apart of course from the nut and screw, which latter element unfortunately entirely depends on workmanship without much possibility of scaling down the resulting errors by the manipulation of the proportions. The bed, that is the moving tube 1, is geometrically held in its slide, and no errors in the latter can prevent the true parallel movement of the microscope's optical axis, whilst the circular cross section of the former is the easiest form to machine truly that could be imagined. Moreover, the distance between its bearing surfaces, over 30 inches as against 3 inches in the Hilger instrument, greatly reduces the angular disturbance due to any irregularity in its form. The tube is correctly constrained; besides which the force of the spring and the opposing force of the screw are in line, and disposed centrally with respect to the bearing surfaces; hence no couples are developed.

As in the case of the "Hilger Measuring Micrometer" I have used one particular "Cambridge Measuring Microscope" for a number of years without observing any deterioration, in spite of the heavy work to which it has been subjected, and the number of different people who have used it.

These two instruments have been described in great detail; partly in order to afford a basis for comparison with other instruments which will be more briefly discussed, and partly because these instruments are excellent representatives of two entirely opposite types of design. One of the firms involved has an international reputation for the excellence of its workmanship, and, in the past, this has perhaps led to an undue reliance on this factor, and so to conservatism in design; the other firm, whilst not neglecting workmanship, has been a pioneer in the newer kinematic and semi-kinematic design, and has been a leading influence in its popularization.

3. ADAM HILGER: NEW MEASURING MICROMETER[1]

This instrument, like the older model, is designed to measure lengths up to 15 cm., and, generally, is fitted to be employed on almost exactly the same class and range of work. In almost every other respect, however, the design departs radically from the firm's older methods—see Figs. 72 and 73. The New Measuring Micrometer is made in two models which are almost identical, except that one has been specially corrected and is guaranteed to be accurate to within higher limits. Thus the ordinary model is stated by the makers to be capable of relative measurement to an accuracy of 1 μ over the whole range, whilst absolute measurement is guaranteed to 0.01% at 62° F. The absolute pitch of the screw is given on an accompanying test sheet to 1 part in 50,000; the greatest permissible error of absolute pitch being 0.01%. The graduated drum reads by vernier to 1 μ. In the case of the specially corrected model the guaranteed limits of error are halved in each case, and the drum reads by double verniers to 0.1 μ.

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[1] For a really good discussion of the principles of measuring micrometer design, and the design of this instrument in particular, see: *A New Measuring Micrometer*" by J. H. Dowell, F.O.S., of the Designing Department of Messrs. Adam Hilger, Ltd. An article in the *Proceedings of the Optical Convention* (England), 1926, Part II. Also reprinted separately.

As can be seen from the accompanying illustrations the microscope is stationary whilst the object carrier slides on the frame; the carrier functions as a slide and the frame as a bed in the sense used earlier. The moving parts are designed on semikinematic principles, for the correct number of forces and locators are employed, whilst the latter are expanded into functional surfaces. Moreover, the distance between the extreme supports of the object carrier is 28 cm. whilst its upper surface is only 4.7 cm.; the proportions are therefore such as to minimize errors arising from irregularities in the three sliding surfaces provided on the bed. These latter are plane and are tested by the firm's well known interference methods.

The screw, which rotates without translation, is supported in bearings at each end, and is, in addition, positioned as regards translation by bearing against a sapphire block at its left hand end (assuming the position of an observer, Fig. 72). Assuming that the thrust surface of the screw end either fails to be accurately at right angles to the axis of the screw, or fails to be centred about that axis, then any de-

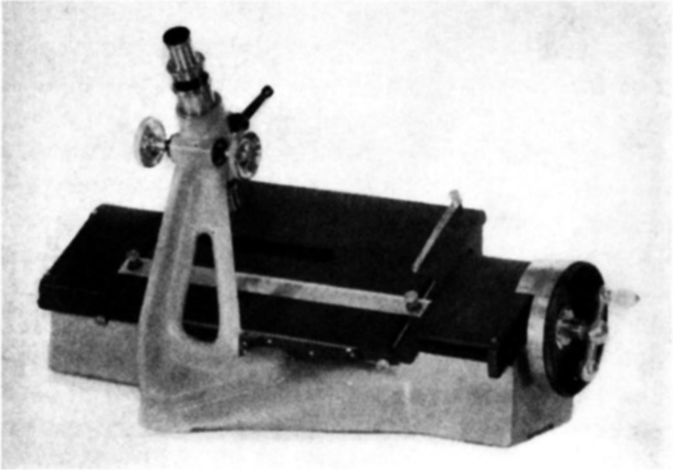

Fig. 72.

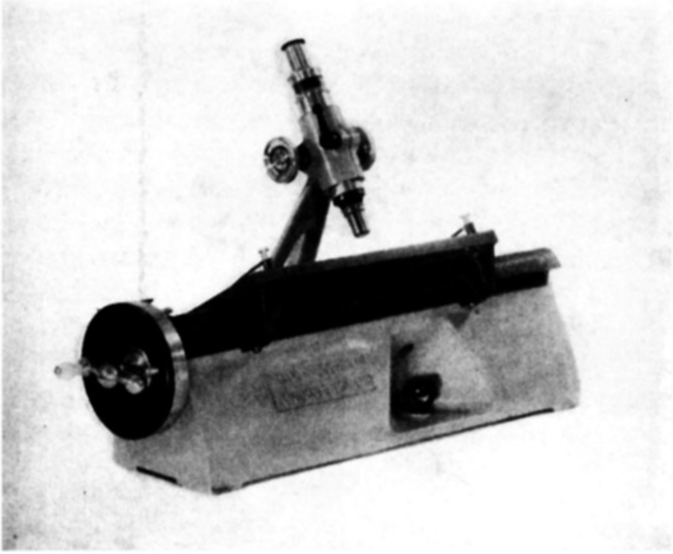

Fig. 73.

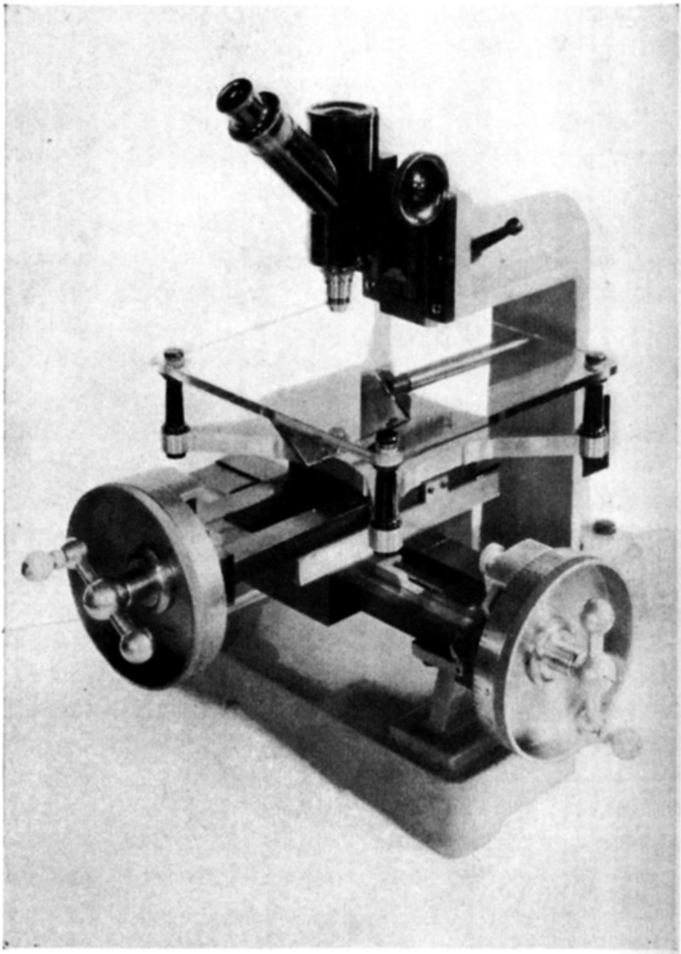

FIG. 74.

parture from the normal to that axis on the part of the sapphire block would result in a periodic error having a periodicity of one turn. To avoid this the block is lined up to the axis of the bearings carrying the screw by a further application of interference methods. The nut, which translates without being permitted to rotate, is not located with respect to the slide except as regards their mutual translation; thus overconstraint is avoided. In addition, means are provided for releasing the nut from the slide to enable the latter to be brought quickly up to its initial position. This nut moreover is relieved of its thread, except in four small areas arranged in pairs at each end 90° apart, and a maintaining force as between the nut and screw is provided in the shape of a floating threaded sector of the nut backed up with a strong spring. The result of this rather radical departure is undoubtedly to prevent side float between nut and screw, which might otherwise introduce a small degree of freedom between the two in the direction of measurement.

Special care has been taken to protect the working parts from dust, and the whole layout is designed to take advantage of the combined benefits due to the firm's almost unique standard of workmanship, together with those of kinematic and well proportioned design. At the same time in minor though important details (as e.g., the microscope rack) the emphasis is still on workmanship rather than kinematic principles, the latter only being introduced where the highest attainable accuracy is required; it would seem less costly in human effort to reverse this procedure, as was done in the case of the last instrument considered.

4. ADAM HILGER: CROSS-SLIDE PHOTO-MEASURING MICROMETER

It is sometimes convenient to measure lengths in two directions at right angles, but this cannot be done by any of the instruments described above; though in the case of the Cambridge measuring microscope a very limited cross range is provided by the micrometer head of one of the microscopes, which can be set at right angles to the length of the object.

Instruments designed to permit of such cross measurements are made, and it might have been supposed that this would be the very occasion when a movement of both the microscope and the object would be a suitable arrangement. In any case two independent movements are involved, and, rather than mount one moving frame on another, a device which is apt to accentuate any incompleteness or error in the elements, it should be well worth while to consider whether the microscope might not move in one direction, and the stage carrying the object move independently in the other. Messrs. Adam Hilger make a "Cross-slide Photo-measuring Micrometer" which is practically identical with their earlier instrument already described, one bed being mounted at right angles on the slide of the bed below. There is one significant difference, namely, that the upper slide carries the object stage instead of the microscope which is now fixed. Figure 74 shows a general view, and it can be seen that this instrument is virtually a duplication of the other one. The details of the screws and length of travel are the same in both cases, as well as the beds and slides. The horizontal stage is a sheet of glass to permit of light being transmitted from the mirror below. It will be noticed that the optical axis of the microscope has been bent by a prism system to permit of a natural position of the observer's head.

The possible sources of error are, for the most part, those we have already considered in the older instrument, with the proviso that, where two moving systems are mounted one on the other, the danger of an incomplete element is doubled for each motion. There is, however, one entirely new possi-

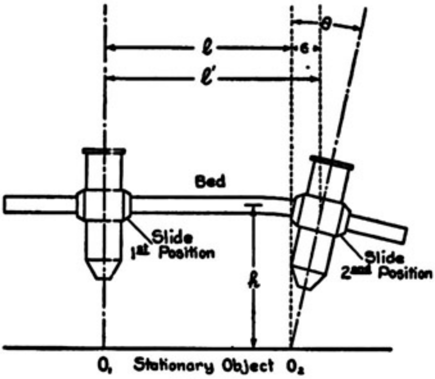

FIG. 75.

bility of error that, within narrow limits, was not significant in the case of the single measuring instrument; namely, the danger of the slide being out of truth in a direction at right angles to its direction of travel; this may not be very important to the measurement performed by that slide, but it is likely to be vital to the measurement at right angles, in the case where the first slide is moved in order to bring the second object mark of the other slide into the field of view. This leads to the consideration of the best position for the slides relative to the object to be measured. Figure 75 represents the case in which the moving member carries the microscope. The length to be measured is

$$O_1 O_2 = l;$$

but owing to the curvature of the stationary bed the slide has actually travelled over the length l'. If the measurement is taken by the operating screw this will, or should, be at the same level as the slide to relieve the latter of any mechanical couple; hence the length l will be recorded as l', and

$$\epsilon = l' - l = h \operatorname{Sin} \theta,$$

where h is the distance between the slide and the object in the plane generated by the optical axis of the microscope; and θ is the angle between the two positions of the optical axis when setting on O_1 and O_2. This error can evidently be eliminated by making

$$h = O;$$

in which case moderate lack of truth in the bed, such as might occur compatibly with good workmanship, would cause no sensible error. The same result will be obtained if the case of a moving object under a fixed microscope be considered, and also in the case of lack of truth at right angles to the travel in a cross-slide instrument; but in the last instance it is the other slide that should have $h=O$. To be precise it is not the distance between the object and slide, but the distance between the object and line of measurement that should be zero; and in those instruments having an independent measuring device this is not the same thing. Glancing back at the instruments already reviewed, it will be noticed that no attempt whatever has been made to approximate to this condition except in the case of the new Hilger photo-measuring micrometer. In the case of a cross-slide instrument the design would be helped in this respect if the microscope and object each had one motion, one slide being just above, and the other just below, the object plane. Though, with some loss of compactness, there is no great difficulty in arranging for the slides to be in the plane of the object in either single, or cross-slide instruments. It is easy to see that only lack of truth in the bed would cause error, whatever the value of h. The form of the slide being immaterial, whether bed or slide be the fixed member, for the latter always uses the same bearing surfaces; hence it is important to arrange for the member with the best form for accurate machining to be made the "bed". None of the instruments so far reviewed violate this condition.

5. SOCIÉTÉ GENEVOISE: COMPARATOR OF ASTROGRAPHIC PLATES

Figure 76 illustrates this very interesting cross measuring instrument, which differs in many particulars from those already considered. Like the Hilger cross-slide photo-measuring micrometer, it can take measurements in two direc-

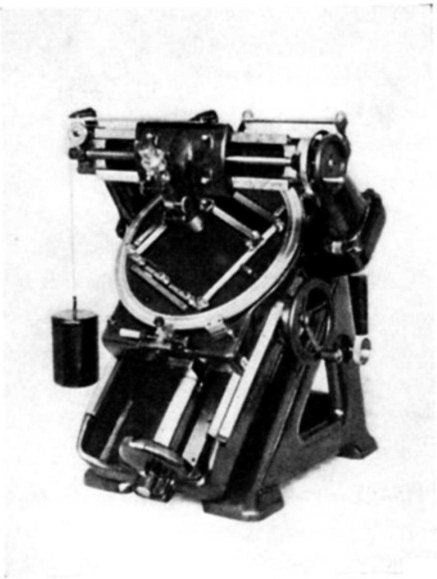

Fig. 76.

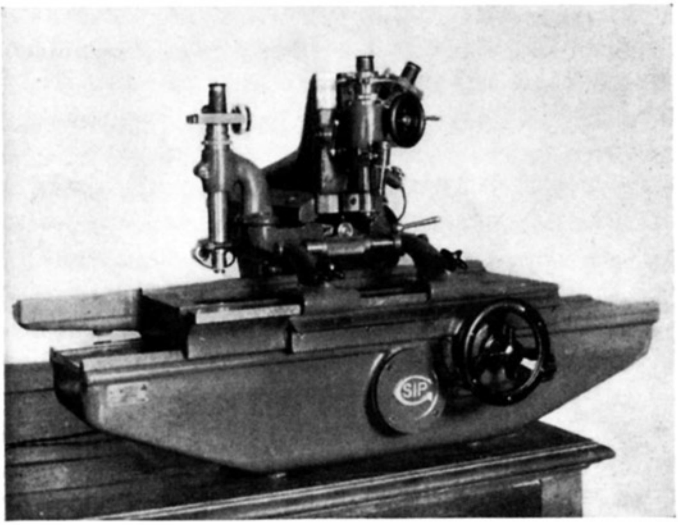

Fig. 77.

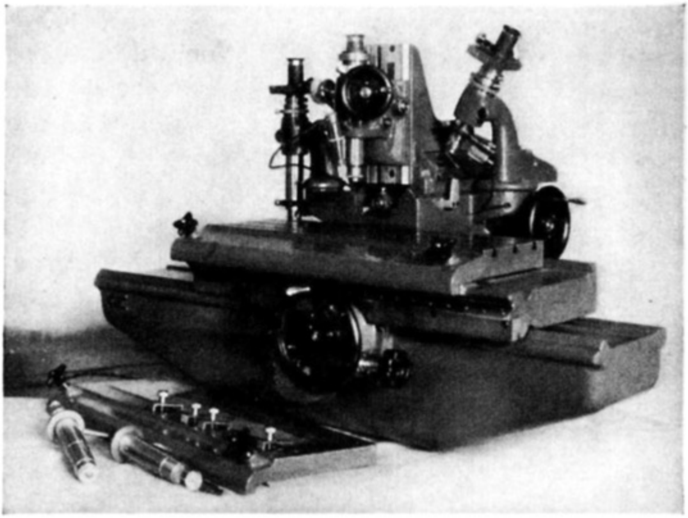

Fig. 78.

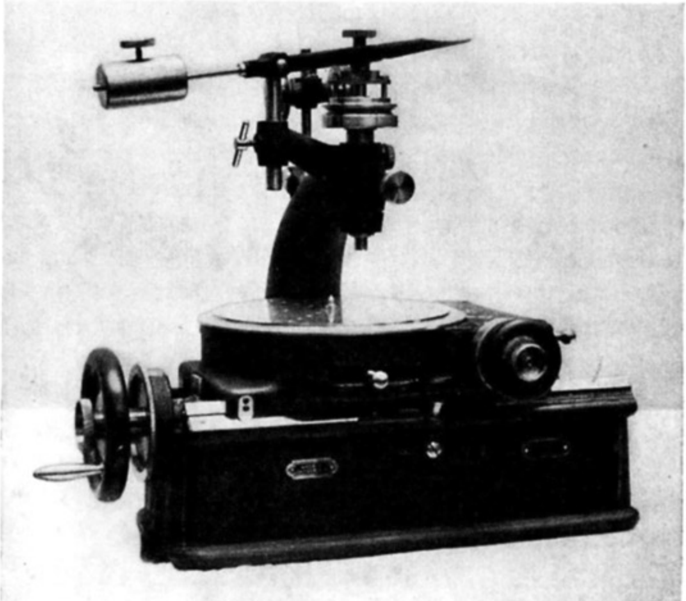

Fig. 79.

tions to a least count of 1 μ; but, in addition, the object under measurement is capable of a rotation which can be read by vernier to one second of angle. The possibility of measuring angle as well as length enables this instrument to be put to a wide variety of uses, other than that from which it derives its name; and, although it is essentially designed for measuring photographic plates of some sort, even this restriction could be avoided by the use of special adaptors to hold other objects.

As can be seen from the illustration one bed lies just below the object plane of the microscope and mounts a slide carrying the work, whilst the slide on a second bed above the object plane carries the microscope. Thus both microscope and object move in directions at right angles to each other; and this has the advantage, as compared with the Hilger instrument, of enabling both slides to be fairly close to the object plane, and also avoids loading the movement of one slide with any lack of rigidity possessed by the other. In the case of both translations the screws are held in compression, not by a spring as in the Cambridge instrument, but by the force of gravity. The weight shown on the left of the illustration loads the microscope slide. The object slide is loaded by its own weight, partially relieved by a weight behind which cannot be seen; this weight is less than sufficient to overcome the influence of gravity on the slide itself. This method of loading the slides has the merits of simplicity, compactness for a given travel, and preserves a uniform load in all slide positions; but it is somewhat less convenient than a spring load, especially where the instrument has to be moved from one position to another.

In order to ensure that the friction between slide and bed shall not oppose the loading of the screw all "cuts" are taken with the hand controls turned in a clockwise direction. The screws are held in journal bearings at both ends; and that end is provided with a thrust block which results in the screw being under compression; the other end floats as regards end play. Thus the microscope screw carries its thrust block at the left hand end, and the other screw at the lower end towards the front of the instrument. It will be noticed that the latter screw carries its recording drum at the bottom of the instrument, but not its actuating handle, which is placed conveniently at the right hand side on a shaft geared to the screw shaft. This gearing incidentally prevents the necessity for rotating the handle anticlockwise when performing a cut, and so keeps the two handles in line in this respect. The two slides are clearly of very different size and weight, and, although the tension on the two screws can be kept equal by a proper choice of counter weights, nevertheless the inertia, stiction, and friction of the lower slide must be greater than the other; this will necessitate the application of more force on the handle. This could have been countered by gearing down the handle actuating the lower slide, but then a given hand movement would not produce the same effect in the image space which would have been objectionable. Actually the lower handle is designed with a greater crank length and a larger diameter wheel for final "cutting".

Both screws are fully constrained, semikinematically, and the design of the traversing nut is so arranged as not to add either to these constraints or to those of the slides (except in the direction of motion). The loading of the slides eliminates all backlash between nut and screw, and presses a fork-shaped piece, fast to the slide, against its nut; the latter is free to rotate and translate in any direction at right angles to the motion so far as the slide is concerned. The unwanted translations of the nut are prevented by its fit on the screw, whilst its rotation is eliminated by providing the nut with a finger projection which presses against a bar substantially parallel to the axis of the screw. Instead of making this bar strictly plane on its working surface it is curved to counteract small errors in the screw, and so, by inducing a limited rotation in the nut, functions as a "correcting bar". In general appearance this excellent instrument is rigid and compact, although the general lines are not conspicuously clean. Moreover, the comfort of the observer has been studied, both as regards the general slope of the instrument (which determines the direction of sight), the position and design of the control handles, and also the position of the recording drums. Gravity loading of the slides is, as mentioned above, a mixed blessing; but the fact that the slides, nuts, and screw are loaded assists in the elimination of incomplete elements and is good. Inadequate attention seems to have been given to protection against dust and dirt generally, both beds and screws being fully exposed. Semikinematic design has been skillfully used in vital parts and results in strength and rigidity combined with complete elements and absence of serious strain, though there is perhaps a tendency to rely unduly on workmanship rather than design in some parts.

6. Société Genevoise: Universal Measuring Apparatus

In complete contrast to the last instrument, the same firm manufactures another type of cross-slide measuring instrument (see Figs. 77 and 78), and it will be seen that this differs essentially from those already described.

In the first place we have here a cross between a machine tool and a measuring instrument; for the central microscope of the three shown (Fig. 78) is located in the tool holder, and can be replaced by tools as required. The possibility of using this apparatus as a precision machine tool explains its extreme robustness and an almost complete reliance on machine tool design, as opposed to the lighter kinematic layout. At the same time this apparatus is particularly designed to measure "in rectangular coördinates all kinds of work pieces such as forming tools, profile gauges, templets, cams, small jigs, thread cutters, dies, etc. Micrometer screws, thread gauges and worms can also be tested for pitch, profile angle, and diameters by means of an optical measuring process", to quote the makers.

It can be seen that both the microscope and object are mounted on slides; the former taking care of the transverse motion with a range of 10 cm., and the latter effecting longitudinal motion with a useful travel of 40 cm. This arrangement is in line with the previous instrument considered, but, in this instance, the recording devices are entirely divorced from the actuating mechanism, and consist in each case of a subsidiary microscope, reading directly onto a straight scale. The microscope on the extreme left of the illustrations looks after the position of the longitudinal slide, and that on the right the transverse slide. In both cases readings are given to 1 μ.

Figure 77 illustrates the use of centre supports which can be clamped to the longitudinal slide, whilst Fig. 78 shows the alternative use of a T-slotted worktable. Besides these, a measuring table with a glass plate, permitting of illumination from below, is provided for miscellaneous measurements. This very fine apparatus will not be considered in detail as it is not quite in line with the type of instrument under review; it has been included on account of its interesting features, particularly the divorce of actuating and recording mechanism, made desirable by the length of travel in one case, and also by its weight, which arises from the dual use for which the instrument has been designed.

7. SOCIÉTÉ GENEVOISE: MACHINE FOR MEASURING POLAR COÖRDINATES

The following instrument (Figs. 79 and 80) made by the Société Genevoise, like the previous one, is included to illustrate the diversity of designs which are possible, and which naturally arise from a consideration of the variety of uses to which measuring microscopes may be put. As its name implies this instrument, which is of the single slide type, is adapted for the measurement of polar coördinates as well as for measurement of lengths. Although no cross-slide is provided, measurements in any direction (within two dimensions) can be made either by polar or rectangular coördinates through a suitable use of the micrometric turntable.

The slide, the screw, and its constraints, and the nut with its correcting bar, are designed on exactly the same general lines as the "Comparator of Astrographic Plates" made by this firm and detailed above; consequently these particulars need not be further described. The slide is spring loaded to maintain the screw under compression, and to eliminate play as between the various working parts; the means for doing this enable a relatively large travel (10 cm.) in proportion to the length of the instrument. A steel ribbon, fast to the slide, is led over a drum controlled by a coiled spring in such a way as to maintain the ribbon under tension. This is not only compact but also permits of a very small variation in the loading as between the various slide positions. The makers guarantee an accuracy of 5 μ on the drumhead reading, effected by means of vernier.

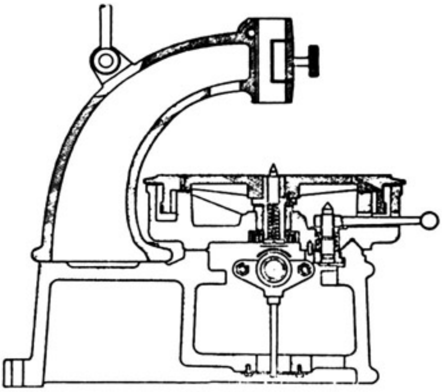

FIG. 80.

The turntable, as Fig. 80 clearly shows, is supported on a central spindle, and is actuated by a worm and wheel; to the former is attached the drumhead which records angles to an accuracy of 20 seconds.

Like all the work of this firm the instrument is designed on sound engineering lines; semikinematic principles being employed only where necessary to obtain the desired accuracy rather than to reduce cost. Great rigidity is obtained, and the instrument has a generally pleasing appearance, though sufficient protection is not afforded to the working surfaces; and, as in the last instrument reviewed, the observer is constrained to look directly downwards when using the microscope.

I am informed by the company that they have discontinued the manufacture of this interesting instrument, and this suggests a lack of demand for measurement by polar coördinates. Have the lack of facilities for lighting from below, combined with the rather low limits of accuracy guaranteed by the makers, contributed to this dearth of orders?

8. ALFRED HERBERT: MICROSCOPIC MEASURING MACHINE

The last instrument I shall describe is the "Microscopic Measuring Machine" made by Messrs. Alfred Herbert, Ltd., of Coventry, England, the well known makers of machine tools, etc. See Figs. 81 and 82.

This instrument is a departure from most of those previously reviewed. In the first place it is larger; for the measuring machine weighs between two and three hundred pounds. Then it has a long range, being capable of measuring lengths up to 12 inches (30.5 cm.), with a least count of 0.0001 inch or 2.5 μ. But apart from modifications necessitated by its greater size and weight, the great dissimilarity between this and some of the other instruments is caused by a difference in the work for which it is intended.

All the instruments described have been designed with a

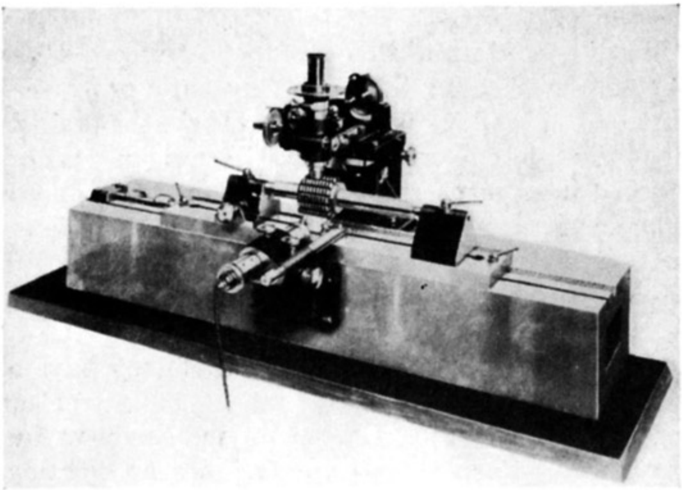

FIG. 81.

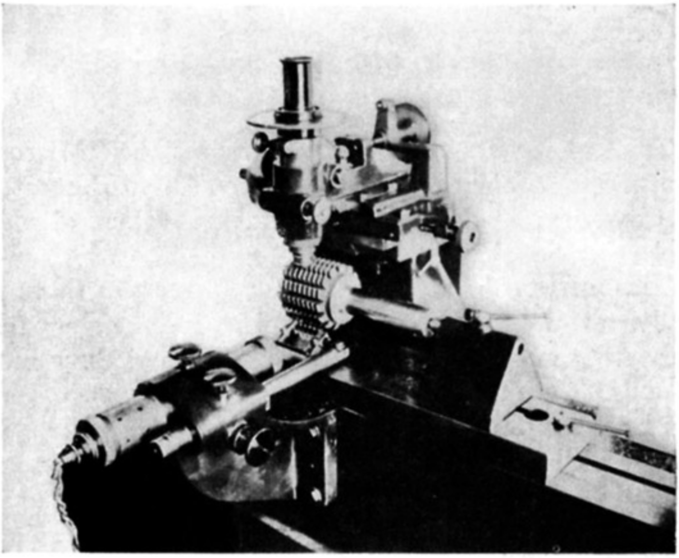

FIG. 82.

view to a wide general utility; but it is clearly impossible for any one to cover adequately all the possible uses for which a measuring instrument might be required. Consequently in practice the designer will have in mind some one duty, or class of work, for which his instrument is preëminently intended; and then will devote considerable thought in adapting his original design to enable the instrument to cover as many allied measurements as can be managed. Thus, both the name and the design of the Hilger photo-measuring micrometers, and this firm's cross-measuring instrument, suggest that these were primarily designed to measure photographs, and more particularly negatives requiring transmitted light. A positive is rarely used in such work as every additional process tends to degrade the image and adds to the risk of error due to shrinkage. The new design of the Hilger measuring micrometer seems to possess very similar facilities.

The Cambridge instrument, on the other hand, is definitely ill adapted for measuring negatives, in that no provision is made for transmitted light; this is curious since it would have been very easy to make the necessary alterations, and this class of work is very common in most laboratories. Perhaps it was thought that the short travel (40 mm.) made the instrument rather useless for this purpose in any case, and it is not difficult to mount the negatives independently if it be desired. The microscope heads show that this instrument was primarily intended for measuring small opaque objects, such as are used equally in laboratories and in shops making small precision tools and instruments. For instance, the diameter of indentations made in the Brinell hardness test; the pitch of small screws; angles between parts to moderate accuracy; the distance between parallel lines engraved on a metal surface (as in checking an engraved scale), and a host of similar measurements. The possibilities of the Herbert Measuring Machine are not very dissimilar except that the variety of its work is rather more restricted, resulting in a design more closely adapted to its work. The objects suitable for measurement are somewhat more bulky and heavy such as: cutting tools; gauges and templets; simple lengths; pitch of threads; depth of threads, etc. In fact, this instrument is designed to meet the needs of firms manufacturing machine tools, and similar accurate parts; and covers somewhat the same field as The Société Genevoise's Universal Measuring Apparatus.

Turning to Fig. 81 it will be noticed that both the object and the microscope move independently; the latter being mounted on cross-slides, whilst, as regards the microscope, the cross-slides are actuated and measured by rotating and translating micrometer screws carrying reading heads; the nuts being fast to their respective frames. These two slides are carried on a third which is capable of moving on its bed nonmicrometrically and across the length of the instrument; this assists the initial setting up.

The actuation and measuring gear of the object stage are independent, neither involving a screw, and the measurement is performed in an interesting manner. The box bed and sliding stage or table, to use the maker's term, each carry a hardened flat contact piece (left hand end) between which is placed a hardened steel measuring rod of known length, and the table is moved to the left until the rod and contact pieces are in contact. A range of measuring rods is supplied giving the table a possible travel of 12 inches in steps; each step being less than the travel of the microscope subdividing the interval. This device does not make for very rapid measurement, as it would be necessary to change the measuring rod and reset the table in the case of lengths greater than the scope of the slides. Nevertheless, it has great advantages in the circumstances. In the first place, the table is necessarily heavy owing to the weight of the objects mounted, as e.g., in the illustration; and moreover the relatively long travel of 12 inches was required; consequently the lead screw, if combined with the measuring gear, would have had to be heavy and very expensive, and would moreover be subject to considerable wear. Two alternatives suggest themselves: first that a separate measuring screw be provided, but its length would still make it expensive, and, with so long a traverse, the saving in time would not be great; similar remarks apply to the other possibility, that the table should remain stationary and one of the microscope's slides have a 12 inch movement. The measuring rods are relatively cheap, can be tested at intervals and replaced when worn; besides no change in their length need lead to error provided that length be known. If the instrument is habitually to be used in measuring small deviations from some one length a corresponding measuring rod could easily be made or obtained to minimize the travel of the microscope, and thus cut out the possibility of proportional errors in the lead screws.

It can be seen that the table is fitted with adjustable centres, one having a cross adjustment, but these are removable, and the table could be used with special fittings for special work. Artificial lighting and an adjustable mirror are provided; and, by fitting the table suitably, a wide range of work could be performed; though naturally a smaller instrument would be more convenient for some classes of work. As regards the general layout, it will be realized that this instrument is designed by a maker of machine tools, and intended for a similar class of user, consequently it departs widely from laboratory practice; especially little or no reliance has been placed in fully or semikinematic design; the various elements follow the practice of machine tool design, for which this firm has a wide reputation. The various links are substantial and ample bearing surface is provided, reliance being placed on workmanship. The relative merits of this type of design undoubtedly rise with the size and weight of the mechanism; though that would hardly apply to the microscope component, which is really an average sized small instrument in itself. Even in the case of the table it would seem as though a semikinematic arrangement might achieve the same result with less human labor.

Every type of firm has its own particular style of design which has proved especially suitable for a given range of work, and there is a tendency to carry the same methods over to all work in which the particular firm may engage; and with good reason, for the practical detailed knowledge which has been acquired in one style would have to be relearned if another were attempted.

It will be noticed that the height of the microscope slides could be dropped to a level more nearly corresponding to that of an average object with advantage, the microscope still being held at its present height to enable the same sized objects to be accommodated.

Range of Measuring Microscopes

The eight instruments just described have been selected as representing very different approaches to closely analogous problems; and in every case the solution has resulted in an instrument of considerable merit which has won for itself a reputation. This list does not, of course, begin to be exhaustive, and many beautiful designs by leading makers have had to be omitted for lack of space; and, although some of these are similar in many respects to those described, others would show marked differences; for measuring microscopes range from standard table microscopes with a micrometer attachment in either the eyepiece or the stage, to enormous and costly apparatus for comparing lengths up to several metres, resting on concrete foundations, and provided with constant temperature tanks and other refinements; this last, however, is an extreme instance of a special purpose instrument.

VARIETY IN DESIGN

Considering the instruments described above it will be seen that they differ from each other in a variety of ways. There are variations such as were considered at the beginning of the chapter; the microscope may be held stationary and the object move, or vice versa; both may move in directions mutually at right angles; or, as in the Herbert "Microscopic Measuring Machine", both may travel in the same direction, one in predetermined and the other in graduated lengths. Similarly the relative motions of rotation and translation as between nut and screw can be differently arranged. Several means of recording the lengths involved are employed; and so forth.

Secondly, the theoretical basis of design can be varied between those depending for their merit primarily on quality of workmanship assisted by good proportion, and those depending on a combination of "kinematic design" and good proportion. A defense for either basis can be made depending on circumstances, but in either case good proportion is an ingredient; no defense can hold which seeks to excuse an absence of this quality.

STYLE

A third sort of difference between nominally similar instruments depends on a quality which is best described as "style".

Differences of style are permissible and even desirable, for most situations can be adequately met in a large variety of ways; and these style variations illustrate the folly of the old adage which would have us believe that there are two ways of doing a job—the right way and the wrong way. Each of these major alternatives cover an infinity of possibilities.

Style is an expression of the individuality of the designer, his training, and the tradition of his environment; and is partly expressed by his individual balance as between a number of desirable qualities for which he is more or less consciously striving; cost, particular facilities, degree of accuracy, pleasing appearance, strength, rigidity, compactness, wearing qualities, ease of control, simplicity of construction and of manipulation, are some of these. Many of the qualities desired are only compatible within the narrow limits; and a due balance depends partly on the factor of style, and partly on a more systematic decision as to the precise field for which the instrument is intended. This latter also affects the theoretical basis of design as between a kinematic and other design; the latter becoming more suitable as the weight and size of the parts increase. Even here style is often a predominant factor. Thus the Cambridge Instrument Company designs from a background of light accurate instrument experience, Messrs. Alfred Herbert from a background of high class machine tool work; hence when the latter mounts a light microscope on a slide the design approximates closely to that of machine practice, whilst the former makes the utmost use of kinematic principles. Thus a design corresponds both to the designer's bent, and to the accepted methods of the workshop for which he is designing. This propensity to make a general prototype fit all occasions can very easily be carried too far, but has obvious advantages in practice. In particular, the use of nonkinematic design is often an expression of conservatism rather than a considered choice.

The introduction of style into commodities, so definitely intended for utilitarian ends as an instrument, is an interesting example of the often forgotten fact that it is quite impossible to find such an absurdity as an unstyled creation. The choice open to a designer is not as to *whether* his instrument shall be styled, but *what* style he shall create; and moreover a "style" attitude in reality implies a sort of balance between a large number of variable qualities which are difficult to discuss quantitatively. For this reason style is an important aspect of design and merits close attention, both on the part of the designer and the prospective purchaser.

CHAPTER XII

THE HUMAN FACTOR

Some instruments are entirely automatic, and perform their function without human aid; thermostats are frequently so designed. Other instruments do depend on human intelligence for their proper functioning or interpretation, but the human actions do not present the "bottle neck" to the final result. A recording barometer, for instance, depends on its record being subsequently read. It is a fact, however, that the errors associated with this instrument are not caused by any difficulty on the human side. The record can easily be read with an accuracy at least as great as is warranted by the instrumental accuracy itself, or by the meaningful interpretation which can be given to the figures so obtained. In the case of yet other instruments the human factor can, and often does, put up the most serious errors involved. An instrument designer must accept humans as he finds them, but he can often minimize the human errors by a skillful design of his instruments. The human becomes, in effect, part of an instrument in two chief ways. First, because of his intelligence; for he is literally the brains of the instrument; he has opinions, and comes to decisions. Secondly, the human is often called on as a source of power; and, more usually, where this power is to be applied with delicate discrimination in accordance with the opinions he has formed, or is then and there forming.

These two activities, judgment and physical action, are continuously interacting when an instrument is in the hands of an observer; the first mainly raises psychological problems, whilst in the case of the latter these are mixed with more physiological considerations.

JUDGMENT

It has already been seen that all preserving agencies were not material, and that, in the case of an observer using a measuring microscope, one of the preserving agencies was a human judgment. This consisted in judging whether a graticule line had been symmetrically placed with respect to a line on the measured object. It will be remembered that this judgment was materially affected by the direction in which the graticule line was moved onto the object line; if the graticule line moved up from the left, the observer judged that it was symmetrically placed when actually on the right side of the object line, and vice versa. The direction of movement of the measuring line affected the judgment of its position at rest; although, logically, the previous movement was quite irrelevant to the question.

This illustrates one of the most important of human characteristics as they affect instrument designers, *namely the habit of using all possible sensations, relevant and irrelevant, when coming to an understanding of any sort.*

We spend our whole time sizing up situations without any knowledge as to how our opinions are formed. On entering a room the relative distances of the various objects are all roughly known within a second or two; we distinguish the tables from the chairs; know which materials are hard and which are soft, and are able to conduct ourselves with sense and discrimination, in what really are complex physical conditions; and all this without ever giving the subject a moment's conscious thought. This implies a tremendous amount of synthesis of which we are not directly conscious; moreover for the most part we cannot turn our attention to this synthesizing process even when we would. This is a prime condition of life; for it would be quite impractical to make a thousand and one bodily adjustments on the basis of consciously solving mathematical problems. Imagine the plight of any one in the driving seat of an automobile whose "road sense" was entirely dependent on conscious processes of logical deduction; the situation would be ridiculous. Exactly the same thing applies to all our knowledge of the external world; the data on which it is based lies buried, for the most part, beneath our consciousness, and only the final outcome attracts attention. The behavior of living organisms is dependent on habits of synthesis to an incredible degree; it applies at all levels from the highest judgments to the lowest neural reactions, and is dealt with at length in many psychological and physiological treatises.

For our purposes it is sufficient to recognize the existence of this universal habit; it is an unparalleled device for enabling living organisms to react to their environment with reasonable speed and certainty; but, from the standpoint of a scientist, it has an obvious drawback. For we never quite know what factors are significantly affecting our judgment on any given point; and, even when we do know, we can do very little about it.

Thus, the experiment with the graticule and object lines informs us that our opinion of their relative positions is noticeably affected by the direction in which the one has moved towards the other. From the standpoint of measurement, this movement is utterly irrelevant, and it should be disregarded; unfortunately this unwanted act of synthesis is performed at some level well below our conscious thought and is quite out of control. Since the instrument designer cannot well design a graticule line which can be brought onto its object without obviously moving it up from somewhere, it follows that this very important source of error must be tolerated to a certain extent. Two things can be done. The observer can bring up the moving line alternatively left and right; and the designer can so arrange his instrument as to facilitate this. Also the designer can provide the microscope with sufficiently low powers, and this is very often not done. For it will be remembered (Chapter VI) that, as the power of the microscope is dropped, short period errors increase, but that biased and erratic errors tend to diminish. Since the latter, though easily the most serious, often remain hidden unless the measurement is very thoroughly conducted, there is always a probability that the observer will use a dangerously high power to diminish the very noticeable short period errors; this probability becomes a practical certainty in the usual case where no suitably low power is provided. On a thoroughly second rate object a power of ×6 is often at least as high as should be used; a skilled observer can often attain an average precision per setting of 2 or 3 μ with this, even when measuring a poor object. On occasion it is wise to be satisfied with a lower power than this, but the observer will be singularly fortunate if he has been given the opportunity of doing so.

"Common sense" is based on a synthesis of all that is experienced at the moment, together with past experience; and, being well below the level of consciousness, our common sense opinions and judgments cannot easily be modified to meet unusual circumstances. This is apt to be a standing danger in the case of instruments which depend on an observer experiencing an unusual combination of sense impressions; and this is more particularly true of optical instruments. One example has already been given and some more follow.

It sometimes happens that a unit power telescope has to be incorporated into an instrument; that is a telescope which does not alter the apparent size of the object. The point of using such a telescope, rather than plain sight, is usually to project cross wires, or graticule lines, onto the plane of the object; this can only be done with a lens system of some sort. Usually in such a case advantage is taken of the optical system to introduce a higher magnification, but in certain circumstances this is inadvisable, either because of other optical requirements, or sometimes merely in order that the object shall appear to the observer in its usual guise.

Now the interesting point is that an object seen through a unit power telescope always looks smaller than as seen by naked sight. It is not easy to understand why this should be so, and the explanation is complex. In the first place objects tend to look smaller when one eye is shut, and the ordinary telescope is monocular. Then it may be that the loss of brightness when seen through the lens system assists this illusion. If an object be looked at through a narrow tube (a sheet of paper rolled into a half inch tube) it is very apt to look smaller than when seen by one eye without the tube. This last illusion is not very easily explained; it has something to do with the loss of field, and may be due to a lack of comparison with other objects. If a number of people are asked how large the full moon looks when it is high in the sky, most mention some object ranging in size from a pea to an orange, and very few think the moon looks as large as a large plate. Now the moon and an orange subtend about the same angle at the eye ($\frac{1}{2}°$) when the latter is at a distance of 10 or 12 yards. This perhaps suggests that, when no easy means exist for judging the distance of an object, it is automatically imagined at some arbitrary and rather close distance, and its size judged accordingly. A tube, by blocking out surrounding objects, prevents an easy judgment of distance, and so may cause distant objects to be judged nearer, and so smaller, than they really are.

Probably all these causes, and others, assist in building up the illusion of diminished size caused by a unit power telescope. At any rate, if the reason for the power is to make the object look familiar, it is wise to use a power of ×1¼ to ×1½, and this is sometimes done.

A very good illustration of the paralyzing result of sensations which conflict with common sense can be arranged with a stereoscope. These instruments are used for a number of purposes, but the following experiment can be tried with a simple stereoscope such as is sold as a toy for a small sum. A number of pairs of photographs are supplied with these toys, each pair representing substantially the same view, or object, but taken from opposite ends of a short distance or "base length". If these be placed in the stereoscope, each one in front of its corresponding eye, the fused picture appears to stand out in bold relief.

Instead of placing each photograph in front of its corresponding eye they may be transposed, so that the right eye views the picture intended for the left, and vice versa. Then considerations of geometry show that the whole "relief" should be reversed, so that those objects which originally appeared nearest now seem to be the most distant; the fused picture has been turned inside out. So long as no absurdity results, as where geometrical patterns are substituted for actual photographs, this reversal is plainly seen; but, if representations of some natural view be transposed, the mind revolts at lamp-posts appearing more distant than the walls they clearly cover, houses turned inside out, and roads whose widths subtend a greater angle as they retire into the distance; and so forth. Consequently, although personally I find no difficulty in the legitimate use of a stereoscope, I have never succeeded with a natural picture reversed; and this is the usual experience. No amount of argument, or effort of will, prevents the observer from synthesizing his whole experience, even where it will obviously prove a hindrance to the present circumstances.

It is just this inveterate habit of making use of our whole experimental knowledge, even when logically irrelevant or worse, which makes the stereoscopic range finder of such doubtful value, in spite of its obvious advantages.

A stereoscopic range finder performs two main functions. First it employs optical means to enhance the perception of depth, or distance, and so makes this sense more precise at considerable distances; then it arranges for some mark ("wander mark") to appear in the sky above the objects to be ranged. This mark is so arranged as to appear nearer or further, by a simple adjustment, and is set at the apparent distance of the object. Finally the adjusting device for the "wander mark" is graduated in yards or metres, from which the distance of the object is obtained.

Now distance is appreciated by a large variety of means. Thus human beings are not only binocular, but also the two images are stereoscopically combined; that is to say, the two visual representations of the outer world are combined into one picture so far as our consciousness is concerned; this results in the sensation of distance. Some one object will be directly looked at by both eyes (the "point of fixation"), and the respective positions on the two retinas are called corresponding points; but other objects nearer or further than the point of fixation will be received on "disparate" points of the two retinas. This may cause a consciousness of change of distance rather than a double image. Again, as the point of fixation is changed from one object to another, the angle between the optic axes will alter, depending on the two distances involved; this, by means of kinesthetic sensation, leads to an estimate of distance. This is the most obvious means we possess for judging distance, and has the merit of being geometrically precise; consequently it is made use of in stereoscopic range finders. Nevertheless there are many other ways by which this judgment is assisted and these are not all accurately known. At distances beyond stereoscopic discrimination, or when one eye is closed, an estimate of distance (often surprisingly accurate) can be made.

We estimate such distances or depths by differences in distinctness, in size, or in light and shade. The illusions of distance which occur in exceptionally clear or foggy weather, show to what an extent we are habitually dependent on these "psychological factors". Their importance is further instanced by the observation that the stereoscopic effects of a painting are far more striking in uniocular than in binocular vision. The two eyes, with their usual accuracy, insist that the canvas is flat; the influence of size, color, light and shade is more successful in suggesting relief when the painting is viewed by a single eye.[1]

In addition to these there are probably innumerable more or less subtle distinctions which enhance the sense of distance; e.g., change of color, absence of rustling noise in the country from distant foliage, loss of detail (as opposed to distinctness) resulting in different characteristic patterns for similar objects, the immobility of distant trees compared to near ones in a wind; distant landscapes are often referred to as "peaceful", and so on.

It is no argument against these latter methods of estimation that they are not effective because others previously mentioned are more dominant and more accurate; this would be to commit the old mistake of supposing our apprehension to be logical instead of synthetic.

Every possible sense contributes in every possible way to enhance the force, the affective tone, and the "security" of an impression; they act as buttresses to that sense which can supply the best quantitative data.

This, perhaps, can be made more clear by the following illustration. If, by some deductive means, we know that the weight of a body lies between 400 and 700 tons, and by another similar method we can show that it lies between

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[1] C. S. Myers, *Experimental Psychology*, p. 267.

100 and 1,000 tons, the latter method has not added to our knowledge: this is logic. But if one sensation suggests the first limits (400–700) and another sensation acting alone suggests the second limits (100–1,000) then the two acting together may suggest finer limits than either, say 500–600 tons. Even if the second sensation does not lead to closer limits it will greatly add to the security of belief.

Reverting to the faculty of estimating distance, it is not certain even at very close ranges exactly which sensations are dominant, although those connected with binocular vision are no doubt the most important, and at the shortest ranges perhaps eye accommodation becomes effective too. There is one important distinction between these various sense data, namely, that those depending on binocular vision rest on geometrical properties which vary accurately with the distances involved according to some law, whilst such changes as the increasing indistinctness, and predominance of the blue end of the spectrum with distances, are only average effects subject to the widest fluctuations; consequently binocular instruments for the direct estimation of distance are designed to make use of the "geometrical sensations".

Very roughly stereoscopic sensation becomes ineffective at some distance such as 300 to 400 metres;[1] but in stereoscopic range finders, and similar instruments designed to measure distances, often amounting to several miles, this difficulty is overcome by artificially increasing the stereoscopic effect to such an extent that the two images of the most distant object required will possess the geometrical stereoscopic properties corresponding to a distance of a few feet. Thus, the average interocular distance for men is in

[1] I am aware that Professor Myers gives the limit of stereoscopic perception of distance as about 20 metres (*Experimental Psychology*), and I am loath to disagree with so eminent an authority, but my experience is as stated above; perhaps we are referring to slightly different things. Stereoscopic range finders are used at distances which would be quite impossible on the basis of Professor Myers' figure.

the neighborhood of 65 millimetres, and so a range finder with a base of say 2 metres will have an enhanced geometrical stereoscopic effect, due to this cause alone, of $\frac{2{,}000}{65}$, or roughly 30 times. In addition a magnification of perhaps ×10 will be used which increases the geometrical stereoscopic effect to 300 times as great as that of a man using his naked eyes. If we assume that, for micrometric work, the unaided vision can be sufficiently trusted up to about 100 feet, then, on the face of it, a two metre range finder's limit becomes 30,000 feet, or roughly 6 miles.

Various methods have been devised for turning the stereoscopic relief so obtained to micrometric account; but they all depend on the same basic principle, and the following may be taken as typical of the rest. Suppose that in a pair of field glasses two identical graticules be fitted, one for each eye, and that the graticules instead of being marked with crossed lines have some mark as this: ▼; then each eye, when looking through the instrument at a distant object, will receive a similar impression of the mark, just as they receive similar impressions of the distant objects.

If the relative arrangement of the two marks is suitable the two eyes will fuse them into one image, which will appear to be at some definite distance depending on the relative positions of the retinal images. Thus if one of the graticules is controlled by the observer, and he moves it slightly towards the other graticule, the fused image appears to approach him; similarly when the distance between the two graticules is increased the fused image appears to recede. Since the apparent distance of the fused image is a function of the horizontal distance between the two graticule marks, the controlling device for the latter can be graduated directly in yards, to correspond with the apparent distance of this "wander mark". Then if the observer sights on an object known to be 100 yards distant and turns his graticule control until it reads "100 yards" the wander mark will appear above the object, and at exactly the same distance from him. Conversely, if an object is sighted and the graticule control actuated until the wander mark appears to be at the same distance as the object, then the distance of the latter can be read off the control graduations. This instrument is a stereoscopic range finder.

In reality stereoscopic instruments are not quite so satisfactory as the above description might suggest; for although almost every one can judge the equality of distance of two objects at a distance of a few feet, yet, when presented with the same parallax in a stereoscope, it is common knowledge that many people fail to see any relief in the field of view. Of those that do see this, many fail to project the wander mark into the outer space, it obstinately refuses to leave the plane of the graticules; and a very small minority are of the slightest use for micrometric work even after extended practice and instruction. Moreover even the best stereoscopic observers are conscious of a certain artificiality in the "illusion", and, if flustered or "off color", their ability temporarily deserts them. This is very unlike our ordinary appreciation of distance, for it is difficult to imagine that any ordinary degree of discomfort would prevent the average man from clearly appreciating that an object 50 feet away was nearer than one 70 feet off; nor is the appearance of "depth" in his surroundings an illusive sensation requiring careful nursing. This difference, between a stereoscope and naked sight, lies in the fact that the former does violence to the observer's instincts; the usual volley of sensations which normally accompany his point of fixation or his field of view have been seriously modified, with the natural result that the required judgment either refuses to be evoked, or is uneasily held.

For instance the observer is presented with a parallax which usually accompanies an object at say 20 feet; but he is, in fact, ranging on a man at say 6,000 feet with power

×10, so that the man has an apparent size due to a distance of 600 feet, but he has color and haze effects due to 6,000 feet (plus a little more due to loss in the glass). The observer normally has an extreme horizontal field of view of nearly 180°, but his apparent field has been reduced to at most 40° and is bounded by a hard circle, as though cut out of a black wall. Then a most unnatural object, the wander mark, hovers in the sky and shows no signs of getting dimmer, changing color, or becoming more hazy in outline as it appears to recede. Moreover, since its absolute size remains constant, it has the appearance of getting larger as it recedes; nothing more improbable could be imagined. In a dozen other ways the picture violates "common sense", and the very fact that most of these absurdities are not consciously noticed only increases the subconscious doubt and confusion.

This is an excellent illustration of a beautiful instrument which is largely spoilt because it involves the observer in a number of contrary judgments; and, although only one is logically revelant, the others cannot be abandoned at will. In some degree this danger is almost always present in any instrument in which the human factor forms the bottle neck to accuracy.

Much scientific observation consists in recognizing equality of some characteristic as between two objects. Thus, when a graticule line is registered on an object line, symmetry is sought for; which really implies a recognition of equalities. When two similar objects have to be made equal in some respect, and recognized as such, it is unfortunately impossible to arrange for them to be precisely similar in all other respects; thus, they occupy different situations in the field of view, and may well show other differences, great or small. These logically irrelevant differences are very apt to have an unexpected influence on judgment. For instance it is well known that one of the disturbing factors in visual photometric measurements is the (logically irrelevant) color difference between the two illuminants compared. Again, if the field of a measuring microscope is not uniformly illuminated over its whole extent, this is very apt to influence the final readings.

PHYSICAL ACTION

The examples just given all illustrate the influence of apparently irrelevant sensations on a judgment intended to refer to some one definite sensation; and they have all been taken from the field of optics. This is not because sight is the only sense with which irrelevant sensations get mixed; but sight, being the best developed human sense, is almost exclusively used in scientific observation unless that observation is mixed up with an application of force, when other senses come into play. In the latter case the effect of unwanted sensations in judgment is mixed up with physiological considerations. A few instances will be considered involving hand control.

Many observers when bringing up a graticule onto an object deliberately overshoot it, and on the return run overshoot it to a lesser extent and so on, approaching the mark as a lightly damped pendulum approaches its position of rest. Care is taken to ensure that the final run onto the mark shall always be in the same direction, in order to take up any backlash in the same way on every observation. This may help to reduce the lack of balance so far as graticule travel is concerned, and accustoms the mind to the problem before the final decision is taken. If the backlash present in the instrument is noticeable, this is apt very seriously to disturb the observer's peace of mind when performing the above operation; perhaps this is partly due to the fact that backlash destroys any simple relation between the muscular movement of the fingers and the apparent movement of the graticule. Hence, even when mechanical accuracy is not involved, backlash should always be avoided in a delicate control. Similarly a stiff or harsh control will adversely affect the observer's performance, even if he is easily able to control the movements to within significant limits, by distracting his mind with unwanted signals. So intimately is the co-ordination of hand and eye effected that it is not unusual to find that what was supposed to be an "eye setting" has in fact been partly performed by the hand. Thus, for the sake of greater precision, a graticule may be set 10 times on the object in quick succession and each "cut" recorded; it will often be found that if, when upsetting the cut, the hand never relaxes the control head, the readings will be more concordant than if the hand releases the control head each time. In the former case, the eye having been used exclusively for judging first setting, the hand tends to return subsequent settings to the same muscular condition; the readings, though apparently satisfactory, are largely valueless, as the original setting has really been reproduced rather than the observation repeated.

The importance of not ignoring the hand when performing delicate visual judgments was well understood by Guild in the case of his researches into visual acuity and errors referred to in the last chapter. It will be remembered that the arrangement involved moving a large brass ring, supporting wires; the whole mounted on a trolley 5 metres from the observer and representing a graticule. This graticule was moved by string led round suitable pulleys; the string was wound round a drum which the observer rotated to actuate the graticule, but Mr. Guild states in his account that such an arrangement leads to the observer having to exert a far greater muscular effort than was usual in such settings (i.e., the setting of an actual microscope or optical instrument), and that this might reduce the comparative value of the results obtained; consequently he devised an auxiliary mechanism which results in the observer actuating the usual type of control head with the usual muscular exertion. Fig. 83, which is largely self-explanatory, illustrates the principle of the device. The knurled head *H* is the control actually handled by the observer, and, if this is turned anticlockwise (as seen from the left), the string S_1 is tightened and S_2 relaxed, and so the trolley travels left. Conversely an opposite rotation of *H* will result in the trolley travelling right. The strings S_1 and S_2 on leaving the drum D_2 each take two or three turns round the drum D_1 on their way to the trolley. D_1 is constantly rotated by the electric motor *EM*, and the friction between the drum and the strings is in general small; but if either string is tightened this friction becomes sufficiently great for D_1 to drive the trolley. The drum D_1 is really a capstan, and acts as a relay to the drum D_2, which latter controls the extent and speed of the trolley's excursions but is relieved of most of the attendant work. This very ingenious device proved a complete success and provided a light and smooth control for a comparatively heavy member.

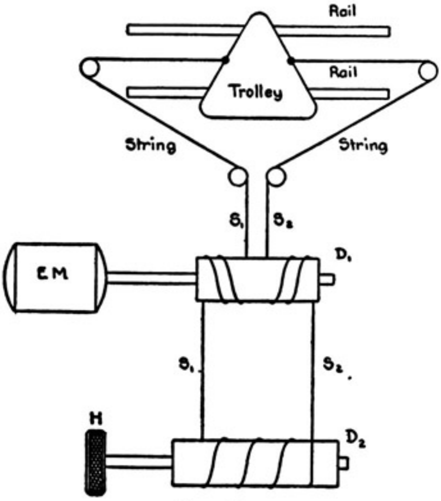

FIG. 83.

In any observation the whole body and mind of the observer is unavoidably involved and, of course, it is impracticable to secure that the influence of every possible sensation shall be beneficially or at least neutrally directed, though more can be done than is often realized. The next best thing is to secure the greatest possible degree of uniformity, or "even handedness", in all external sensory sources and to endeavor to reduce these sensations to a minimum. The best way to reduce irrelevant sensation to a minimum is to be thoroughly comfortable. If the observer has not the sense to be comfortable when he can the designer cannot be held responsible, but it is very easy to design an instrument which puts all hope of comfort out of the question; controls may be so placed that the observer's elbow cannot rest comfortably on the table, or the reach demanded may be inconveniently great. Again after each operation the observer should not have to shift his position in order to read the recording mechanism. Designers of optical instruments are particularly apt to secure a stiff neck for the observer by an inconvenient angle of the optical axis; in particular, instruments incorporating microscopes should *not* have the axes of the latter vertical; man was not designed to spend his life looking at his toes.

All this is apt to be underestimated by designers as involving small matters of detail, but this neglect can ruin a good instrument; nor is comfort always easy to secure and is sometimes the most difficult problem in connection with an instrument, especially in the case of one which is to be used in the field or at sea. The important point is that comfort is not a matter of pleasure but a condition of accuracy, for comfort is an essentially negative state—the absence of irrelevant, rather than the presence of pleasurable, sensation. The importance of reducing unwanted nerve signals to a minimum cannot be overestimated; for, since their coördination or synthesis with the relevant signals is not only out of control, but largely unconscious, it is impossible to predict what their result on the final observation will be; and moreover the disturbance is apt to combine two most deadly characteristics: bias and fluctuation; they cannot be wiped out by repeated readings owing to the former, or cancelled out by differential observations because of the latter characteristic; though a combination of both methods does usually reduce them. A skilled observer, before settling down to a critical observation, and especially where great visual acuity is involved, seems to a novice to spend an undue time "fiddling" with his environment; pulling blinds over bright windows, padding his chair, arranging rests for his elbows, and planning the exact position of his pencil and pad if he is recording his own observations (he won't do this if he can find an assistant), playing with the optical illumination, and generally behaving like a fussy old woman. This behavior is based on sound experience, and is conducive to the "precise frame of mind"; a mind at ease, with a maximum of concentration on the wanted nerve signals and an "even handed" minimum of the rest.

Thus unwanted sensations are dangerous to accuracy; their asymmetry with respect to the wanted sensations are more dangerous still (whether they seem relevant or not). It is an immediate inference that any difference between experimental conditions, and those they are intended to simulate, must be presumed to be relevant until proved otherwise.

So far we have been considering the dangers arising from the habit of synthesizing sensations and experience generally; but it is a necessary condition of skillful action and has its advantages too. For in many ways the observer can be trusted to act better than could be expected from a consideration of what he knows. Particularly in the case of delicate movements he is apt to show great (if unconscious) ingenuity in circumventing his apparent limitations.

For instance it is not infrequently required to arrange for the angular adjustment of some apparatus such that it can be positioned to about one second of angle; this particularly applies to optical instruments; and it would seem natural to provide some particularly fine and expensive adjustment; but experience goes to show that this is rarely needed. Thus imagine an instrument weighing perhaps 40 or 50 pounds with its three legs standing on a hole, slot, and plane, respectively. One of these legs takes the form of a stout bolt screwed through a threaded hole in the base of the instrument with its rounded end standing on the plane. See Fig. 84. The instrument can be tilted as desired about the line joining the other two legs by screwing this bolt up or down in its threaded hole. Suppose the pitch of the thread on the bolt be 16 to the inch, and the distance between this leg and the line joining the other two legs be 12 inches; then one revolution of the bolt forming the adjustable leg will tilt the instrument about $\frac{1}{3}$°, or in round figures a tilt of one second will necessitate the bolt being rotated $\frac{1}{1200}$ part of one turn.

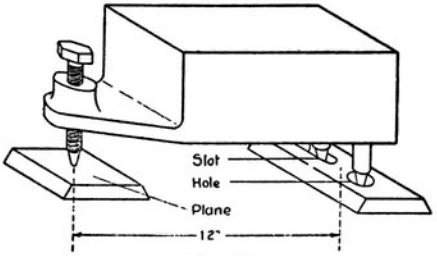

FIG. 84.

I have used precisely this device for levelling such an instrument to this degree of precision, turning the bolt with an ordinary spanner, and find that it presents no difficulties. Of course this simple arrangement does not record the angle tilted, some other device being used for this purpose. Nevertheless it will be found that, unless the action of the bolt in its thread is unusually smooth, it is not possible by simple pressure on the spanner to start and stop the rotation of the bolt within such fine limits as $\frac{1}{1200}$ of a turn (or little more than $\frac{1}{4}$ of a degree). Such a fine motion is quite unthinkingly produced, by any one used to handling tools, by slightly bumping the spanner with the hand; for, although muscular pressure cannot be started and stopped sufficiently quickly to produce a small enough motion of the spanner, nevertheless the kinetic energy stored in the hand can in practice be controlled within fine limits by varying the speed with which the hand bumps the spanner, and frequently by adding a small muscular force, in itself insufficient to maintain the rotation of the bolt.

Again, senses are divided into two groups according to whether they do or do not display the quality of "projicience", that is the quality of appearing to project the sensation away from the body onto the object of origin. Sight provides the outstanding example of projicience; thus looking at a red flag it is the *flag* that seems red not our retina, we see the flag over *there*, not here. The sensation is "flag over there" and it has no apparent connection with our bodies. Similarly a moderate sound usually seems to be located in its place of origin though often, especially when loud, it is partly in our ears. The location of smell is often divided between the object of origin and the smell receptors in the body, sometimes it is entirely located in the latter but rarely wholly in the former. The senses of the hand (touch, pressure, etc.) have a great significance to the designer and observer, for, on occasion, they show some power of projicience along a tool or material part held by the hand; they seem to indicate the far end of this extension. It is as though the tool had become one with the hand, and the latter's nerves had grown along the tool. This is an experience common to every one, and is particularly obvious in the case of a well practiced tool such as a pen; thus, as I am writing these words, the feel of the operation is largely composed of an almost direct consciousness of the friction and sliding of the nib over the paper; the junction of my pen and hand giving rise to little sensation. Moreover this condition is more or less essential to the most skillful use of the tool, and is the common state of skillful workmen. This projicience from the hand is more or less prominent according to circumstances, it is particularly vivid in the case of a pen but can be noticeably present even when the hand actuates some mechanism, the results of which are seen, not felt. Here the travelling comparator again provides an example, for the hand travels the microscope and the eye observes a relative change of position between the graticule and the object line; after some experience the "feel" of the movement (which latter has to be delicately controlled) is directly in the object sighted rather than the hand, the latter being almost forgotten, so smoothly does it obey its orders.

Since this power of projicience is of great value in the delicate manipulation of an instrument, it is worth while to consider the conditions favorable to its appearance. These must be such as to cause a minimum of sensory impulses due to the connection between hand and tool, and a maximum due to the operation which that tool performs. Thus, in the case of the pen we should be unconscious of the pressure between pen and fingers, but conscious of the movement and resistances of the pen on the paper, as sensed both through the hand and eye; similarly in the case of the comparator the "feel" of the control head should be a minimum, but the eye should sense the resultant motion with a maximum of vividness. The result in either case is to connect the kinesthetic sensations directly with the ultimate required motions, rather than the intermediate movements of the hand or other links.

Thus, in any form of hand control, the form of grip provided must not require excessive force to prevent slipping of the fingers; any mechanical harshness or vibration in the transmission will be felt by the fingers, and the senses will be projected to the seat of the vibration rather than to the required effect. It is perhaps an unconscious realization of this that makes observers so exacting in their demands for "sweet" controls, and so loud in their condemnation of the slightest harshness; even when, at first glance, the mechanism provided seems fairly adequate. Similarly, backlash or lost motion is a great bar to the immediate association of kinesthetic sensation and final movement.

For the most part delicate manual controls take the form of rotating some member, rather than requiring a direct push or pull. There are excellent reasons for this, based on the type of precision associated with the action of the muscles involved. Unfortunately, so far as I know, no really systematic work has been published on the design of hand controls, and these vary widely in their suitability from one instrument to the next. Such research is badly needed, for it is illogical to design the remaining elements of an instrument with the greatest care, and then leave to chance that element which involves the human factor.

Such information as the following would be useful in the design of rotary controls: the influence on performance caused by the direction of the shaft's axis with respect to the body; the position of the control along the shaft's axis; the throw of the crank handle (if any); inertia (increasing smoothness of action and, possibly, decreasing precision); and so on.

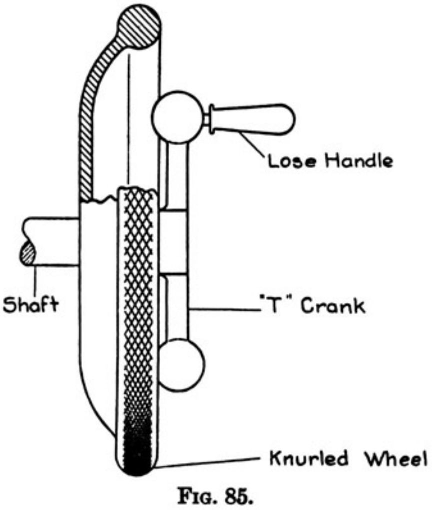

FIG. 85.

As a means for rotating shafts, control wheels, cranks and crank handles, all have their uses. The control sketched in Fig. 85 is convenient for many purposes. The handle is used for quick rotations and the crank itself, or the wheel, for final settings. The wheel can be used to introduce inertia as desired. Almost every alternative arrangement of a hand control, whether of shape or position, involves the use of different muscles, or the same muscles in different degrees; and the precision, force, speed of action, and staying power, will vary in each case.

As a rough rule, precision and delicacy of control is best achieved with the small hand and finger muscles; whilst the larger arm muscles deliver more power and are less easily fatigued.

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